BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. VI.

Containing the Minutes of the Society for the year 1883, and the Minutes of the Mathematical Section from its organization, March 29th, to the close of the year.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON:
1884.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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TREASURER'S REPORT.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who, having complied with Section 13 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.*

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices

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*Under this rule the Mathematical Section was organized March 29, 1883. Its rules and proceedings follow the Bulletin of the General Meeting.

of the Society shall be submitted in writing to the General Committee for approval.

11.* Any paper read before a Section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

12. New members may be proposed in writing by three members of the Society for election by the General Committee; but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

13. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

14. The fiscal year shall terminate with the Annual Meeting.

15. †Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments. They can, however, resume their membership by giving notice to the President of their wish to do so.

16. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

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* Adopted, May 19, 1883. † Amended, Nov. 10, 1883.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5.* If the author of any paper read before a Section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee to be appointed as the Section may determine.

The report of this committee shall be forwarded to the Publication Committee by the Secretary of the Section, together with any action of the section taken thereon.

6. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

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* Adopted May 19, 1883.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 16, 1882.

President	J. W. POWELL.
Vice-Presidents	J. C. WELLING, J. E. HILGARD, C. H. CRANE, J. S. BILLINGS.
Treasurer	CLEVELAND ABBE.
Secretaries	G. K. GILBERT, HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

W. H. DALL, C. E. DUTTON,
J. R. EASTMAN, E. B. ELLIOTT,
R. FLETCHER, WM. HARKNESS,
D. L. HUNTINGTON, GARRICK MALLERY,*
C. A. SCHOTT.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman*, G. K. GILBERT, HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman*, HENRY FARQUHAR, CLEVELAND ABBE, S. F. BAIRD.†

* Mr. Mallery was elected Vice-President October 13 to fill the vacancy occasioned by the death of Mr. Crane. Mr. C. V. Riley was at the same time added to the General Committee to fill its number.

† As Secretary of the Smithsonian Institution.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 22, 1883.

President	J. C. WELLING.	
Vice-Presidents	J. S. BILLINGS.	GARRICK MALLERY.
	J. E. HILGARD.	ASAPH HALL.
Treasurer	CLEVELAND ABBE.	
Secretaries	HENRY FARQUHAR.	G. K. GILBERT.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

H. H. BATES.	E. B. ELLIOTT.
W. H. DALL.	ROBERT FLETCHER.
C. E. DUTTON.	WILLIAM HARKNESS.
J. R. EASTMAN.	J. J. KNOX.

C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* HENRY FARQUHAR. G. K. GILBERT.

On Publications:

G. K. GILBERT, *Chairman.* CLEVELAND ABBE. HENRY FARQUHAR.

S. F. BAIRD.*

* As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to December 31, 1883.

The names of founders are printed in SMALL CAPITALS.

(*d*) indicates *deceased*.

(*a*) indicates *absent* from the District of Columbia and excused from payment of dues until announcing his return.

(*r*) indicates *resigned*.

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Abbe, Cleveland	Army Signal Office. 2017 I St. N. W	1871, Oct. 29
Abert, Sylvanus Thayer	Engineer's Office, War Department. 1724 Penn. Ave. N. W.	1875, Jan. 30
Adams, Henry	1607 H St	1881, Feb. 5
Aldis, Asa Owen	1518 H St. N. W.	1873, Mar. 1
Allen, James	Army Signal Office. 1707 G St. N. W.	1882, Feb. 25
Alvord, Benjamin	1207 Q St. N. W.	1872, Mar. 23
ANTISELL, THOMAS	Patent Office. 1311 Q St. N. W	1871, Mar. 13
Avery, Robert Stanton	Coast and Geodetic Survey Office. 320 A St. S. E.	1879, Oct. 11
Babcock, Orville Elias	2024 G St. N. W	1871, June 9
Bailey, Theodorus (*d*)		1873, Mar. 1
BAIRD, SPENCER FULLERTON	Smithsonian Institution. 1445 Mass. Ave. N. W.	1871, Mar. 13
Baker, Frank	326 C St. N. W	1881, May 14
Baker, Marcus	347 Hill St., Los Angeles, Cal	1876, Mar. 11
Bancroft, George	1623 H St. N. W	1875, Jan. 16
BARNES, JOSEPH K. (*d*)		1871, Mar. 13
Bates, Henry Hobart	Patent Office. The Portland	1871, Nov. 4
Beardslee, Lester Anthony (*a*)	Navy Department	1875, Feb. 27
Bell, Alexander Graham	Scott Circle, 1500 R. I. Ave	1879, Mar. 29
Bell, Chichester Alexander	1221 Conn. Ave. N. W.	1881, Oct. 8
BENÉT, STEPHEN VINCENT	Ordnance Office, War Department. 1717 I St. N. W	1871, Mar. 13
Bessels, Emil	Smithsonian Institution. 1444 N St. N. W.	1875, Jan. 16
BILLINGS, JOHN SHAW	Surg. Genl's Office, U. S. A. 3026 N St. N. W.	1871, Mar. 13
Birney, William	456 Louisiana Ave. 1901 Harewood Ave., Le Droit Park.	1879, Mar. 29
Birnie, Rogers (*a*)	Cold Spring, Putnam Co., N. Y	1876, Mar. 11
Bodfish, Sumner Homer	Geological Survey. 605 F St. N. W	1883, Mar. 24
Browne, John Mills	Medical Director, U.S.N. The Portland.	1883, Nov. 24
Burchard, Horatio Chapin	Director of the Mint. Riggs House	1879, May 10
Burgess, Edward Sanford	High School. 1214 K St. N. W	1883, Mar. 24
Burnett, Swan Moses	1215 I St. N. W	1879, Mar. 29

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Busey, Samuel Clagett	1525 I St. N. W	1874, Jan. 17
CAPRON, HORACE	The Portland	1871, Mar. 13
Case, Augustus Ludlow (*a*)	Navy Department. Bristol, R. I	1872, Nov. 16
CASEY, THOMAS LINCOLN	Lieut. Col., Corps of Engineers. 1419 K St. N. W	1871, Mar. 13
Caziarc, Louis Vasmer	Army Signal Office. 1415 G St. N.W	1882, Feb. 25
CHASE, SALMON PORTLAND (*d*)		1871, Mar. 13
Chamberlin, Thomas Crowder	Geological Survey	1883, Mar. 24
Chickering, John White, Jr	Deaf Mute College, Kendall Green	1874, Apr. 11
Christie, Alexander Smyth	Coast and Geodetic Survey Office. 513 6th St. N. W.	1880, Dec. 4
Clapp, William Henry (*a*)	1416 Corcoran St	1882, Feb. 25
Clark, Edward	Architect's Office, Capitol. 417 4th St. N. W.	1877, Feb. 24
Clark, Ezra Westcote	Revenue Marine Bureau, Treasury Department. Woodley Road.	1882, Mar. 25
Clarke, Frank Wigglesworth	Geological Survey. 1425 Q St. N.W	1874, Apr. 11
COFFIN, JOHN HUNTINGTON CRANE	1901 I St. N. W	1871, Mar. 13
Collins, Frederick (*d*)		1879, Oct. 21
Comstock, John Henry (*a*)	Cornell University, Ithaca, N. Y	1880, Feb, 14
Coues, Elliott	Smithsonian Inst. 1726 N. St. N. W	1874, Jan. 17
CRAIG, BENJAMIN FANEUIL (*d*)		1871, Mar. 13
Craig, Robert	Army Signal Office. 1008 I St. N. W	1873, Jan. 4
Craig, Thomas (*a*)	Johns Hopkins Univ., Baltimore, Md	1879, Nov. 22
CRANE, CHARLES HENRY (*d*)		1871, Mar. 13
Curtis, Josiah (*d*)		1874, Mar. 28
Cutts, Richard Dominicus (*d*)		1871, Apr. 29
DALL, WILLIAM HEALEY	P. O. Box 406. 1119 12th St. N. W	1871, Mar. 13
Davis, Charles Henry (*d*)		1874, Jan. 17
Davis, Charles Henry	Navy Department. 1705 Rhode Island Ave. N. W.	1880, June 19
Dean, Richard Crain (*a*)	Navy Yard, New York	1872, Apr. 23
De Caindry, William Augustin	Commissary General's Office. 924 19th St. N. W.	1881, Apr. 30
De Land, Theodore Louis	Treasury Dept. 126 7th St. N. E	1880, Dec. 18
Dewey, George (*r*)		1879, Feb. 15
Doolittle, Myrick Hascall	Coast and Geodetic Survey Office. 1925 I St. N. W.	1876, Feb. 12
Dorr, Fredric William (*d*)		1874, Jan. 17
Dunwoody, Henry Harrison Chase	Army Signal Office. 1803 G St. N. W	1873, Dec. 20
Dutton, Clarence Edward	Geological Survey. 23 Lafayette Square.	1872, Jan. 27
DYER, ALEXANDER B. (*d*)		1871, Mar. 13
Eastman, John Robie	Naval Observatory. 930 18th St. N.W	1871, May 27
EATON, AMOS BEEBE (*d*)		1871, Mar. 13
Eaton, John	Bureau of Education, Interior Dept. 712 East Capitol St.	1874, May 8
Eldredge, Stewart (*a*)		1871, June 9
ELLIOT, GEORGE HENRY (*r*)		1871, Mar. 13
ELLIOTT, EZEKIEL BROWN	Office of Government Actuary, Treasury Department. 1210 G St. N. W.	1871, Mar. 13
Emmons, Samuel Franklin	Geological Survey. 915 16th St. N.W.	1883, Apr. 7
Endlich, Frederic Miller (*a*)	Smithsonian Institution	1873, Mar. 1
Ewing, Charles (*a*)		1874, Jan. 17
Ewing, Hugh (*a*)	Lancaster, Ohio	1874, Jan. 17
Farquhar, Edward	Patent Office Library. 1915 H St. N.W.	1876, Feb. 12
Farquhar, Henry	Coast and Geodetic Survey Office. Brooks Station, D. C.	1881, May 14
Ferrel, William	Army Signal Office. 471 C St. N. W	1872, Nov. 16
Fletcher, Robert	Surgeon Genl's Office, U. S. A. 1326 L St. N. W.	1873, Apr. 10
Flint, Albert Stowell	Naval Observatory. 1209 Rhode Island Ave. N. W.	1882, Mar. 25
Flint, James Milton	Smithsonian Inst. Riggs House	1881, Mar. 19
FOOTE, ELISHA (*d*)		1871, Mar. 13
Foster, John Gray (*d*)		1873, Jan. 18
French, Henry Flagg (*r*)		1882, Mar. 25
Fristoe, Edward T.	1434 N St. N. W	1873, Mar. 29

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Gale, Leonard Dunnell (*d*)		1874, Jan. 17
Gallaudet, Edward Miner	Deaf Mute College, Kendall Green	1875, Feb. 27
Gannett, Henry	Geological Survey. 1881 Harewood Ave., Le Droit Park.	1874, Apr. 11
Gardiner, James Terry (*a*)	State Library, Albany, N. W	1874, Jan. 17
Garnett, Alexander Young P. (*r*)		1878, Mar. 16
Gihon, Albert Leary	Navy Department. 2019 Hillyer Place N. W.	1880, Dec. 18
Gilbert, Grove Karl	Geological Survey. 1424 Corcoran St.	1873, June 7
GILL, THEODORE NICHOLAS	Smithsonian Inst. 321-323 4½ St. N.W.	1871, Mar. 13
Godding, William Whitney	Government Asylum for the Insane	1879, Mar. 29
Goode, George Brown	National Museum. 1620 Mass. Ave. N. W.	1874, Jan. 31
Goodfellow, Edward	Coast and Geodetic Survey Office. 1330 19th St. N. W.	1875, Dec. 18
Goodfellow, Henry (*r*)		1871, Nov. 4
Gore, James Howard	Columbian College. 1305 Q St. N. W.	1880, Mar. 14
Graves, Edward Oziel (*a*)		1874, Apr. 11
Graves, Walter Hayden (*a*)	Denver, Colorado	1878, May 25
Greely, Adolphus Washington (*a*)		1880, June 19
Green, Bernard Richardson	1738 N St. N. W	1879, Feb. 15
Green, Francis Mathews (*a*)	Navy Department	1875, Nov. 9
GREENE, BENJAMIN FRANKLIN (*a*)	West Lebanon, N. H	1871, Mar. 13
Greene, Francis Vinton	District Commissioners' Office. 1915 G St. N. W.	1875, Apr. 10
Gunnell, Francis M	Medical Director, U. S. N. 600 20th St. N. W.	1879. Feb. 1
Hains, Peter Conover (*a*)	1824 Jefferson Place	1879, Feb. 15
HALL, ASAPH	Naval Observatory. 2715 N. St. N.W.	1871, Mar. 13
Hanscom, Isaiah (*d*)		1873, Dec. 20
HARKNESS, WILLIAM	Naval Observatory. 1415 G St. N. W.	1871, Mar. 13
Hassler, Ferdinand Augustus (*a*)	Tustin City, Los Angeles Co., Cal	1880, May 8
HAYDEN, FERDINAND VANDEVEER (*a*)	Geological Survey. 1803 Arch St., Philadelphia, Penn.	1871, Mar. 13
Hazen, Henry Allen	Army Signal Office. 1416 Corcoran St.	1882, Mar. 25
Hazen, William Babcock	Army Signal Office. 1601 K St. N.W.	1881, Feb 5
HENRY, JOSEPH (*d*)		1871, Mar. 13
Henshaw, Henry Wetherbee	Bureau of Ethnology. P. O. Box 585.	1874, Apr. 11
HILGARD, JULIUS ERASMUS	Coast and Geodetic Survey Office. 1709 Rhode Island Ave. N. W.	1871, Mar. 13
Hill, George William	Nautical Almanac Office. 314 Ind. Ave. N. W.	1879, Feb. 1
Holden, Edward Singleton (*a*)	Madison, Wisconsin	1873, June 21
Holmes, William Henry	Geological Survey. 1100 O St. N. W.	1879, Mar. 29
Hough, Franklin Benjamin (*a*)	Agricultural Department. Lowville, N. Y.	1879, Mar. 29
Howell, Edwin Eugene (*a*)	Rochester, N. Y	1874, Jan. 31
HUMPHREYS, ANDREW ATKINSON (*d*)		1871, Mar. 13
Jackson, Henry Arundel Lambe (*a*)	War Department	1875, Jan. 30
James, Owen (*a*)	Hyde Park, Penn	1880, Jan. 3
Jeffers, William Nicolson (*r*)		1877, Feb. 24
JENKINS, THORNTON ALEXANDER	2115 Penn. Ave. N. W	1871, Mar. 13
Johnson, Arnold Burges	Light House Board, Treasury Dept. 501 Maple Ave., Le Droit Park	1878, Jan. 19
Johnson, Joseph Taber	926 17th St. N. W	1879, Mar. 29
Johnston, William Waring	1603 K St. N. W	1873, Jan. 21
Kampf, Ferdinand (*d*)		1875, Dec. 18
Keith, Reuel (*a*)		1871, Oct. 29
Kerr, Washington Carruthers	Raleigh, N. C.	1883, Apr. 7
Kidder, Jerome Henry	Smithsonian Institution. 1816 N St. N. W.	1880, May 8
Kilbourne, Charles Evans	Army Signal Office. Lexington House.	1880, June 19
King, Albert Freeman Africanus	726 13th St N. W	1875, Jan. 16
King, Clarence (*r*)		1879, May 10
Knox, John Jay	Treasury Dept. 1127 10th St. N. W.	1874, May 8
Kummell, Charles Hugo	Coast and Geodetic Survey Office. 608 Q St. N. W.	1882, Mar. 25
LANE, JONATHAN HOMER (*d*)		1871, Mar. 13
Lawver, Winfield Peter	Mint Bureau, Treasury Department. 1912 I St. N. W.	1881, Feb. 19

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Lee, William	2111 Penn. Ave. N. W.	1874, Jan. 17
Lefavour, Edward Brown	Coast and Geodetic Survey Office. 117 C St. S. E.	1882, Dec. 16
Lincoln, Nathan Smith	1514 H St. N. W.	1871, May 27
Lockwood, Henry H. (*r*)		1871, Oct. 29
Loomis, Eben Jenks	Nautical Almanac Office. 1413 College Hill Terrace N. W.	1880, Feb. 14
Lull, Edward Phelps	Navy Department	1875, Dec. 4
Lyford, Stephen Carr (*r*)		1873, Jan. 18
MacCauley, Henry Clay (*a*)	Helena, Montana	1880, Jan. 3
McGee, W. J	Geological Survey. 512 13th St. N. W.	1883, Nov. 10
McGuire, Frederick Bauders	1306 F St. N. W. 614 E St. N. W	1879, Feb. 15
Mack, Oscar A. (*d*)		1872, Jan. 27
McMurtrie, William (*a*)	Champaign, Ill	1876, Feb. 26
Mallery, Garrick	Bureau of Ethnology. P. O. Box 585. 1323 N St. N. W.	1875, Jan. 30
Marvin, Joseph Badger (*a*)		1878, May 25
Marvine, Archibald Robertson (*d*)		1874, Jan. 31
Mason, Otis Tufton	Columbian College. 1305 Q St. N.W	1875, Jan. 30
MEEK, FIELDING BRADFORD (*d*)		1871, Mar. 13
Meigs, Montgomery (*a*)	War Department. Rock Island, Ill	1877, Mar. 24
MEIGS, MONTGOMERY CUNNINGHAM	1239 Vermont Ave. N. W	1871, Mar. 13
Milner, James William (*d*)		1874, Jan. 31
Morgan, Ethelbert Carroll	918 E St. N. W	1883, Oct. 13
Morris, Martin Ferdinand (*a*)		1877, Feb. 24
Mussey, Reuben Delavan	P. O. Box 618. 508 5th St. N. W	1881, Dec. 3
MYER, ALBERT J. (*d*)		1871, Mar. 13
Myers, William (*a*)	War Department	1871, June 23
NEWCOMB, SIMON	Navy Department. Stoddart Street	1871, Mar. 13
Nichols, Charles Henry (*a*)		1872, May 4
NICHOLSON, WALTER LAMB	1322 I St. N. W	1871, Mar. 13
Nordhoff, Charles	Alpine, Bergen Co., N. J	1879, May 10
Osborne, John Walter	212 Delaware Ave. N. E	1878, Dec. 7
OTIS, GEORGE ALEXANDER (*d*)		1871, Mar. 13
PARKE, JOHN GRUBB	Engineer Bureau, War Department. 16 Lafayette Square.	1871, Mar. 13
PARKER, PETER	2 Lafayette Square	1871, Mar. 13
Parry, Charles Christopher (*a*)	Burlington, Iowa	1871, May 13
Patterson, Carlile Pollock (*d*)		1871, Nov. 17
Paul, Henry Martyn	Naval Observatory. 917 R St. N. W	1877, May 19
Peale, Albert Charles	Geological Survey. 1210 Mass. Ave. N. W.	1874, Apr. 11
PEALE, TITIAN RAMSAY (*a*)	Philadelphia, Penn	1871, Mar. 13
PEIRCE, BENJAMIN (*d*)		1871, Mar. 13
Peirce, Charles Sanders (*a*)	Coast and Geodetic Survey Office. Baltimore, Md.	1873, Mar. 1
Pilling, James Constantine	Geological Survey. 918 M St. N. W	1881, Feb. 19
Poe, Orlando Metcalfe	34 Congress St. West, Detroit, Mich	1873, Oct. 4
Pope, Benjamin Franklin	Surgeon General's Office, U. S. A. 2029 P St. N. W.	1882, Dec. 16
Porter, David Dixon (*r*)		1874, Apr. 11
Powell, John Wesley	Geological Survey. 910 M St. N. W	1874, Jan. 17
Prentiss, Daniel Webster	1224 9th St. N. W	1880, Jan. 3
Pritchett, Henry Smith (*a*)	Washington University, St. Louis, Mo.	1879, Mar. 29
Rathbone, Henry Reed (*a*)		1874, Jan. 17
Rathbun, Richard	Smithsonian Institution. 1622 Mass. Ave. N. W.	1882, Oct. 7
Renshawe, John Henry	Geological Survey. 1221 O St. N. W	1883, Feb. 24
Richey, Stephen Olin	1426 N. Y. Ave. N. W	1882, Oct. 7
Ridgway, Robert (*a*)	Smithsonian Inst. 1214 Va. Av. N.W.	1874, Jan. 31
Riley, Charles Valentine	Agricultural Dept. 1700 13th St. N.W.	1878, Nov. 9
Riley, John Campbell (*d*)		1877, May 19
Ritter, William Francis McKnight	Nautical Almanac Office. 16 Grant Place.	1879, Oct. 21
Rodgers, Christopher Raymond Perry (*a*)	1723 I St. N. W	1872, Mar. 9
Rodgers, John (*d*)		1872, Nov. 16
Rogers, Joseph Addison (*a*)	Naval Observatory	1872, Mar. 9
Russell, Israel Cook	Geological Survey. 1424 Corcoran St.	1882, Mar. 25

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Russell, Thomas	Army Signal Office. 904 M St. N. W.	1883, Feb. 10
Salmon, Daniel Elmer	Agricultural Dept. 1121 I St. N. W.	1883, Nov. 24
Sampson, William Thomas	Naval Observatory	1883, Mar. 24
SANDS, BENJAMIN FRANKLIN (*d*)		1871, Mar. 13
Saville, James Hamilton	342 D St. (La. Ave.) N. W. 1315 M St. N. W.	1871, Apr. 29
SCHAEFFER, GEORGE CHRISTIAN (*d*)		1871, Mar. 13
SCHOTT, CHARLES ANTHONY	Coast and Geodetic Survey Office. 212 1st St. S. E.	1871, Mar. 13
Searle, Henry Robinson (*d*)		1877, Dec. 21
Seymour, George Dudley (*r*)		1881, Dec. 3
Shellabarger, Samuel	Room 23 Corcoran Building. 812 17th St. N. W.	1875, Apr. 10
Sherman, John	1319 K St. N. W.	1874, Jan. 17
SHERMAN, WILLIAM TECUMSEH (*r*)		1871, Mar. 13
Shufeldt, Robert Wilson	Surgeon Genl's Office, U. S. A. 1619 K St. N. W.	1881, Nov. 5
Sicard, Montgomery (*a*)	Ordnance Bureau, Navy Department.	1877, Feb. 24
Sigsbee, Charles Dwight	Hydrographic Office, Navy Department. 3319 U St. N. W.	1879, Mar. 1
Skinner, John Oscar	1739 F St. N. W.	1883, Mar. 24
Smiley, Charles Wesley	U. S. Fish Commission, 1443 Mass. Ave. 1207 11th St. N. W.	1882, Oct. 7
Smith, David (*a*)	Navy Department	1876, Dec. 2
Smith, Edwin	Coast and Geodetic Survey Office	1880, Oct. 23
Spofford, Ainsworth Rand	Library of Congress. 1621 Mass. Ave. N. W.	1872, Jan. 27
Stearns, John (*a*)		1874, Mar. 28
Stone, Ormond (*a*)	Leander McCormick Observatory, University of Virginia.	1874, Mar. 28
Taylor, Frederick William	Smithsonian Institution	1881, Feb. 19
TAYLOR, WILLIAM BOWER	Smithsonian Inst. 306 C St. N. W.	1871, Mar. 13
Thompson, Almon Harris	Geological Survey	1875, Apr. 10
Tilden, William Calvin (*a*)	Army Medical Museum	1871, Apr. 29
Todd, David Peck (*a*)	Amherst, Mass.	1878, Nov. 23
Toner, Joseph Meredith	615 Louisiana Ave.	1873, June 7
True, Frederick William	National Museum	1882, Oct. 7
Twining, William J. (*d*)		1878, Nov. 23
Upton, Jacob Kendrick (*r*)		1878, Feb. 2
Upton, William Wirt	2d Comptroller's Office, Treasury Dept. 810 12th St. N. W.	1882, Mar. 25
Upton, Winslow (*a*)	Army Signal Office. 1441 Chapin St. N. W.	1880, Dec. 4
Vasey, George (*r*)		1875, June 5
Walcott, Charles Doolittle	Geological Survey. 1116 N. Y. Ave. N. W.	1883, Oct. 13
Waldo, Frank	Army Signal Office. 1427 Chapin St. N. W.	1881, Dec. 3
Walker, Francis Amasa (*a*)	Mass. Inst. of Technology, Boston, Mass.	1872, Jan. 27
Walling, Henry Francis	Geological Survey	1883, Feb. 24
Ward, Lester Frank	Geological Survey. 1464 R. I. Ave. N. W.	1876, Nov. 18
Webster, Albert Lowry	Johns Hopkins University, Baltimore, Md.	1882, Mar. 25
Welling, James Clarke	1302 Connecticut Ave.	1872, Nov. 16
Wheeler, George M. (*a*)	Engineer Bureau, War Department	1873, June 7
WHEELER, JUNIUS B. (*a*)	West Point, New York	1871, Mar. 13
White, Charles Abiathar	Geological Survey. LeDroit Park	1876, Dec. 16
White, Zebulon Lewis (*a*)	Providence, Rhode Island	1880, June 19
Williams, Albert, Jr	Geological Survey. 23 Lafayette Square.	1883, Feb. 24
Wilson, Allen D. (*a*)	Newport, R. I.	1874, Apr. 11
Wilson, James Ormond	Franklin School Building. 1439 Mass. Ave. N. W.	1873, Mar. 1
Winlock, William Crawford	Naval Observatory. 733 20th St. N.W.	1880, Dec. 4
Wolcott, Christopher Columbus (*r*)		1875, Feb. 27
Wood, Joseph (*a*)	Asst. Engineer B. & P. R. R.	1875, Jan. 16
Wood, William Maxwell (*a*)	Navy Department	1871, Dec. 2

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
WOODWARD, JOSEPH JANVIER	Army Med. Museum. 620 F St. N.W.	1871, Mar. 13
Woodward, Robert Simpson	Naval Observatory. 1125 17th St. N.W.	1883, Nov. 24
Woodworth, John Maynard (*d*)		1874, Jan. 31
Yarnall, Mordecai (*d*)		1871, Apr. 29
Yarrow, Harry Crécy	814 17th St. N. W.	1874, Jan. 31
Zumbrock, Anton	Coast and Geodetic Survey Office. 455 C St. N. W.	1875, Jan. 30

Number of *founders*	44	
" members *deceased*		36
" " *absent*		55
" " *resigned*		16
" " *active*		149
Total number enrolled		256

ANNUAL REPORT OF THE TREASURER.

WASHINGTON CITY, *December* 31, 1883.

To the Philosophical Society of Washington:

I have the honor to present herewith my annual statement as Treasurer for the year ending December 31st, and to express my regret that owing to absence from the city I was not able to present this report at the proper time on the occasion of the recent annual meeting, December 22d.

By the kindness of Messrs. Riggs & Co. the Society has been enabled to invest in another $1,000 United States 4 per cent. bond, but in this case a few weeks in advance of the regular winter accumulation of its revenue. The balance shown by Riggs' books against the Society is, therefore, with their assent, and in fact at their suggestion, and will probably be met during January.

The total invested fund of the Society is, therefore, $2,500, of which $1,000 is at 4½ per cent. and $1,500 at 4 per cent.

The further assets of the Society consist of unpaid annual dues to the amount of $185 for 1883 and $90 for 1882. The total active membership remains at about one hundred and fifty, and the probable income for the next year may be estimated at $900, nearly all of which will be needed to pay current expenses and the bills for printing Volume VI.

Early in the year one hundred and fifty-five copies of Volume IV and two hundred and eighty-five of Volume V were distributed to the active members and to about fifty domestic and eighty-five foreign recipients.

The list of recipients is a slightly amended copy of that printed in Volume IV of the Bulletin of the Society. Occasional copies of the earlier volumes have been distributed to those whose sets had accidentally become imperfect, or were otherwise entitled to them.

As custodian of the library and property, I have the honor to report that the Society occasionally receives scientific publications by way of exchange with similar organizations. The accession catalogue of the library now includes a hundred and two numbers or titles, being an increase of thirty-one during the year.

By order of the General Council, the Treasurer was in 1881 instructed to initiate the keeping of a record book containing a sketch of the life and services of the individual members of the Society; this record volume is now prepared, and a notice will be sent to each member asking for the necessary data.

Very respectfully,

CLEVELAND ABBE,
Treasurer.

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer, for the year 1883.* CR.

Expenditures.					Receipts.		
Date.	Vou'r.	Check.	To whom paid.	Amount.	From what source.	Amount.	Total.
1883.							
March 1	1	74	Judd & Detweiler, printers	$60 00	Credit by receipts as follows:		
April 21	2	75	Judd & Detweiler, printers	35 50	Balance carried over from Dec. 28, 1882		$521 07
April 21	3	75	Judd & Detweiler, printers	117 08	Annual dues received:		
April 26	4	76	Cleveland Abbe, Treasurer	46 70	and deposited December 23, 1882	$135 00	
June 20	5	77	Judd & Detweiler, printers	11 50	and deposited February 1, 1883	25 00	
Nov. 26	6	78	G. K. Gilbert, Secretary	5 05	and deposited February 6, 1883	125 00	
Dec. 5	7	79	Riggs & Co., $1,000 U. S. 4		and deposited February 14, 1883	50 00	
			per cent.	1,223 75	and deposited March 17, 1883	100 00	
					and deposited May 1, 1883	70 00	
					and deposited November 15, 1883	95 00	
					and deposited November 27, 1883	105 00	
					and deposited December 10, 1883	65 00	
					and deposited December 28, 1883	70 00	
					Interest on invested funds, viz:	$840 00	
					One $1,000 U. S. bond, at 4½ per cent.	45 00	
					One $500 U. S. bond, at 4 per cent.	20 00	
					Total receipts		905 00
					Receipts from all sources		1,426 07
					Over-draft on Riggs & Co.		73 51
			Total	$1,499 58	Total		$1,499 58

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ANNUAL ADDRESS OF THE PRESIDENT.

ANNUAL ADDRESS OF THE PRESIDENT,

J. W. Powell,

Delivered December 8, 1883.

THE THREE METHODS OF EVOLUTION.

In the early history of research attention was chiefly given to phenomena of co-existence. In late years the phenomena of sequence have received the larger share of attention. The investigation of the phenomena of sequence has led to the invention of a number of hypotheses. In the past history of scientific research three of these have each led to a long series of important discoveries. These are the nebular hypothesis, the atomic hypothesis, and the hypothesis of the development of life. The nebular theory is an hypothesis of astronomic evolution; the atomic theory has gradually assumed the shape of an hypothesis of chemical evolution; and the development theory has been elaborated and re-stated as the hypothesis of biologic evolution. The time has come when in all fruitful research evolution in some form is postulated by each investigator in his own field. Yet many scientific men, though admitting the doctrines of evolution in their own special fields, ofttimes reject them elsewhere; and there is some disagreement even among the greatest thinkers as to the extent to which the hypotheses of evolution can be carried, but all postulate evolution in some form and to some degree.

An attempt will be made in this address to point out what is believed to be the fact—that there are three grand classes of phenomena, constituting three kingdoms of matter and representing three stages of evolution; or, stated in another way, that there has been an evolution of the methods of evolution, so that the methods discovered in the first stage have been superseded by those discovred in the second, and these superseded by the methods of the third stage. It is proposed to indicate and, as clearly as possible within the limits of an address, to define, in terms of matter and motion, the three kingdoms of matter and the three methods of evolution. As precedent to the general statement it will be well, therefore, briefly to consider the kinematic hypothesis.

THE KINEMATIC HYPOTHESIS.

That motion is persistent is the kinematic hypothesis. In the early history of research many modes or varieties of motion were directly observed. To account for these motions they were said to be caused by *forces,* and Force was sometimes defined as that which produces motion. Something, therefore, was conceived to exist—not matter, not motion—an existence that would produce motion. Then arose the question, What is Force—this antecedent of Motion? The researches inaugurated from this standpoint led again and again to the discovery that the antecedent of motion is some other motion, and one after another of the so-called "forces" were thus resolved into motions, until at last only gravity and affinity, and perhaps magnetism, remain as unexplained antecedents of motion. But gravity, affinity, and magnetism are included under one term, "*attraction,*" by those who hold that there is yet a *force*—something other than motion which produces motion. Attraction, then, is left. Sometimes these same philosophers speak of "attraction and repulsion." If, then, all forces the actions of which are thoroughly known are resolved into antecedent motions, it is indeed an inductive hypothesis worthy of consideration that the antecedents of the phenomena of attraction and repulsion may also be regarded as modes of motion.

But this hypothesis is reached by another method. It is known that motions may be transmuted from one kind or mode into another. Affinity can be transmuted into motion, and motion into affinity. If we wish to obtain the mode of motion called electricity, we may derive it from mechanical motion through friction, or we may derive it through affinity in the voltaic cell. If we combine a gramme of hydrogen with oxygen, 34,000 units of heat—a mode of motion—are developed. If a gramme of hydrogen be combined with iodine, 3,600 units of heat—a mode of motion—are absorbed. But why introduce single illustrations? A large part of all the powers used by man in the industries of the world are derived from affinity. Affinity, therefore, is the equivalent of motion. By a similar process it is shown that gravity can be transmuted into motion and motion into gravity, and the trasmutation of magnetism into motion and of motion into magnetism is well known.

It is thus seen that while motion may be derived from the so-called forces, gravity, affinity, and magnetism, these so-called force

may also be derived from motion. In all other cases where a mode of motion is transmuted, it is but changed into another mode. It is therefore an inductive hypothesis that gravity, affinity, and magnetism are also modes of motion.

This hypothesis is reached by yet another inductive process. There is a vast multiplicity of properties which bodies present to the mind through touch, taste, smell, hearing, and sight—properties at first explained as occult. During the progress of scientific research, one after another of these properties has been resolved into motion, until at last two remain unexplained—rigidity and elasticity. By those who hold with most tenacity to older explanations of such phenomena, these two remaining properties are attributed to attraction and repulsion; but those who have fallen into the current of modern thought believe that they can be explained as the results of the composed motion of the constituent parts of the bodies which exhibit them, together with molecular impact. That some such explanation will eventually be fully established is highly probable as an inductive hypothesis.

When these various methods of induction are combined they lead to an hypothesis of the highest character, and we may reasonably expect that all forces will ultimately be resolved into motions. The term *force* will still be of value in science, to be used in each case as denoting the antecedent motion.

Intimately related to the kinematic hypothesis is the hypothesis of an ether, which has also been reached by a variety of inductive methods, *i. e.* from converging lines of research. In fact, the kinematic hypothesis and the ethereal hypothesis are identical, the first being stated in terms of motion, the second in terms of matter.

Intimately related to the ethereal hypothesis is the nebular hypothesis, also reached through a series of converging lines of induction.

Every fact that lends probability to one lends probability to all. Thus each strengthens the other. It must be understood that however probable they may be, they are yet hypotheses, and for their complete demonstration the mode of action must be specifically pointed out in each case.

The ethereal hypothesis furnishes the original homogeneous matter in motion from which the various aggregates have been segregated. The nebular hypothesis takes up this matter while it is yet in a

molecular condition and derives from it the more compounded aggregates and their motions, in obedience to the law of the persistence of motion, which is the kinematic hypothesis. Thus there are bodies of men engaged in researches relating to molecular physics, other bodies of men in researches relating to molecular physics and astronomy, and others in molecular physics and chemistry, all of whose researches converge in the kinematic hypothesis. It is therefore reached by a consilience of many inductive methods.

In the statement thus made concerning the kinematic theory there is no attempt to assemble the data on which it rests. Such task could not be performed in an address, as volumes would be needed for their presentation. An attempt has been made simply to characterize the processes of inductive reasoning by which the hypothesis is reached.

If the kinematic hypothesis should be demonstrated, it would be a veritable explanation. The dynamic hypothesis is no explanation. To exhibit this fact it must be briefly analyzed.

Philosophy is the science of opinion, and the philosopher has for the subject-matter of his science the origin and nature of opinions, and he discovers that they may be broadly grouped in three classes—mythic, metaphysic, and scientific. Mythic opinion arises from the attempt to explain the simple in terms of the compound—that is, to explain biotic and physical phenomena by their crude analogies to human activities. Early man, discovering that his own activities arose from design and will, supposed that there was design and will in all function and motion. Through this method of explanation have arisen the mythologies of the world.

But in the early civilization of the Aryan race a multitude of mythic systems were thrown together and studied by the same body of men, originally for the purpose of deriving therefrom the common truth. The resulting comparison and investigation led to the conclusion that they were all false, and in lieu thereof a new system of explanation was invented. These earlier philosophers of the cities of the Mediterranean, while engaged in the comparison of mythologies, were also engaged in the comparison of languages, and they discovered many profoundly interesting facts of linguistic structure, and the intimate relations between language and thought by which the form of thought itself is moulded. These great facts appearing at the same time that mythic philosophy was dis-

solving into idle tales, led to the origin of a new philosophic method. The men of that day supposed that the truth is in the word, and that a verbal explanation could be constructed; that the philosophy of the universe could be based on language; and to them verbal statement was explanation, final and absolute, and being was but ideal.

But metaphysic philosophy was displaced by the increase of knowledge—the development of scientific philosophy. In this system the phenomena of co-existence and sequence are objectively discerned and classified.

This bare statement of the three methods can be made more lucid by an illustration. Unsupported bodies above the earth fall, and such phenomena are seen so often as to challenge every man's attention. Early man, whose mind was controlled by mythic opinions, subjectively knew that if he wished to move a body he must push or pull it, and to him there was no other method of originating motion.

Some years ago I was with a small body of Wintun Indians on Pitt River, the chief tributary of the Sacramento, engaged in the study of mythology. I had gone among the rocks for the purpose of awakening echoes, that I might elicit from my dusky philosophers an explanation thereof. Unexpectedly I fell upon an explanation of gravity. We had climbed a high crag, and I sat at the summit of the cliff with my feet overhanging the brink. An Indian near me, who could speak but imperfect English, seemed solicitous for my safety, and said: "You better get out; hollow pull you down." I had previously been intent on watching the operations of his mind for the purpose above mentioned, and this expression seemed to me strange; and it started a line of investigation which I eagerly pursued. I soon discovered that he interpreted the fall of bodies by purely subjective analogies. He who stands on a rock but slightly elevated above the earth feels no fear, but if standing a thousand feet above the base of the cliff, he attempts to look over, fear curdles his blood, and he seems to be *pulled over*. As he climbs a lofty pine, at every increase of altitude there is an increase of fear, and he seems to be pulled down by a stronger force. When he rests upon the solid earth he feels no "pull," but when elevated above it he interprets his subjective feelings as an objective pull. Vacuity is personified and believed to be an actor.

In the early winter of 1882 I was with a party of Indians in the

Grand Cañon of the Colorado. Some of the young men were amusing themselves by trying to throw stones across a lateral gorge. No one could accomplish the feat, though they could throw stones even farther, as they believed, along the level land. Chuar, the chief, explained this to me by informing me that the cañon *pulled the stones down.* The apparent proximity of the opposite wall was believed to be actual, and vacuity was personified and believed to exert a force.

Metaphysic explanations of gravity are found. By that method an absolute up and down is predicated, and bodies have a tendency to fall down. This is an explanation in words, the words expressing no meaning but believed to be themselves thoughts. It is perhaps the earliest form of the metaphysic explanation of gravity. But with the progress of knowledge the absolute disappears, and positions are found to be but relative; there is no absolute up and down; and other facts with regard to gravity are discovered. And finally the metaphysician says bodies *attract.* Now the term *fall,* as used by the early metaphysicians, was the name of a motion observed, and it was held to be a complete explanation as long as up and down was supposed to be absolute, not relative; and the explanation was abandoned as insufficient when the ideas of absolute up and down were abandoned. But the word *attraction* does not involve this error. It is simply a name for the phenomenon, without the manifestly fallacious implication of "up and down." And it is a good name for the specific phenomenon to which it is applied. But it must not connotate any other idea; in so far as it does, it is vitiated. Yet the metaphysician will suppose that by using the term "attraction" he explains gravity. The scientific philosopher uses the term purely as the name of the phenomenon, and does not suppose that thereby the phenomenon is explained; and having named it, he still seeks for its explanation—that is, he still seeks to resolve that which is manifestly a complex phenomenon, exhibited in the relations of positions of bodies, into its most simple elements. Whenever this is done he will say that attraction, or gravity—they being synonyms for the same phenomenon—is explained.

The kinematist uses "attraction" as a synonym for "gravitation." The dynamist uses "attraction" as a verbal explanation of Gravitation. The mythic philosopher uses the term to connotate the still further idea that bodies exert a "pull" on one another; and this

latter concept is no less mythic than that of the Indian who believes that the vacuity between them exerts the pull.

It is fortunate for science that every discovery and every inductive hypothesis is rigidly criticised, as this leads to the careful examination of the verity of facts discerned and of the legitimacy of hypotheses derived therefrom. Against the kinematic theory of force much good rhetoric has been hurled, which may be somewhat imitated in the following manner:

Here is a quotation from Bagehot, with an interpolation of my own: "This easy hypothesis of special creation [occult force] has been tried so often, and has broken down so very often, that in no case probably do any great number of careful inquirers very firmly believe it. They may accept it provisionally, as the best hypothesis at present, but they feel about it as they cannot help feeling as to an army which has always been beaten; however strong it seems they think it will be beaten again."

The venerable gentlemen who constitute the elder school tell us that motion is not persistent; that *energy* constitutes a class of things including two groups, the forces on the one hand and the motions on the other; that the total amount of energy is persistent, but that the total amount of motion is changeable. And by their definition force is that which produces motion, *i. e.* force can create or destroy motion. But manifestly where there is more motion there must be less force, therefore force can destroy itself; and when there is more force and less motion, force can create itself.

The moon that passes through the sky of the gentlemen of the old school is moon from the eastern to the western horizon. Then the dragon, which exists not, destroys the moon and thus creates itself, and passing through the cave from west to east it mounts to their horizon, and in the twinkling of an eye commits suicide by creating a moon. It is not strange that the thaumaturgics of such philosophy should lend signal aid to its rhetoric.

The use of hypothesis in science is not only legitimate but an absolute necessity. The science of psychology, as distinguished from metaphysic speculation, points out this fact: that all increase of knowledge is dependent upon hypothesis. Objective impressions made by the phenomena of the universe upon the organ of the mind are discerned only by the aid of comparison, and are added to knowledge only by being combined with previously discerned phe-

nomena. Phenomena imperfectly discerned are such as are combined by superficial analogies; phenomena clearly discerned are such as are combined by essential homologies. With all discernment, therefore, there is comparison, and comparison is reflection and reflection is reason. Now, scientific research is not random observation and comparison, but designed discernment and classification; it is research for a purpose, and the purpose is the explanation of imperfectly discerned phenomena. Phenomena not understood, because imperfectly discerned and classified, are made the subject of examination by first inventing a hypothetic explanation of the same. With this, the investigator proceeds to more careful observation and comparison, devising new methods of discrimination and of testing conclusions. Under the impetus of this hypothetic explanation, discernment and comparison proceed, and additions to knowledge are made thereby, and it matters not whether the hypothesis be confirmed or overthrown.

On this rock much research is wrecked. When an hypothesis gains such control over the mind that phenomena are subjectively discerned, that they are seen only in the light of the preconceived idea, then research but adds to vain speculation. A mind controlled by an hypothesis is to that extent insane; the rational mind is controlled only by the facts, and contradicted hypotheses vanish in their light.

There is another rock on which research is wrecked—the belief which ofttimes takes possession of the mind that the unknown is unknowable, that human research can penetrate into the secrets of the universe no farther. It is the despondency of unrewarded mental toil.

Yet another rock on which research is wrecked is the definition of the unknown. Phenomena appear, but whence is not discovered, and resort is had to verbal statement, and the verbal statement oft repeated comes to be held as a fact itself. This is the vice of all metaphysics, by which *words* are held to be *things*—spectral imaginings that haunt the minds of introverted thinkers as devils possess the imaginations of the depraved.

In the midst of the sea of the unknown stand the three rocks: the controlling hypothesis, the unknowable unknown, and the verbal definition, and in the waters about them are buried many wrecks.

COMBINATION OF MATTER.

When the various bodies known to mind are resolved into their constituent parts to the utmost of art and knowledge, such parts are found to be so minute as almost to disappear in the perspective toward the infinitesimal. The molecular bodies thus dimly discerned are combined and re-combined, until substances are produced that come distinctly within the cognizance of our senses, so that we are able to observe their forms and motions. These molar bodies are again combined, until at last bodies of such magnitude are produced that they are but dimly discerned in the perspective toward the infinite—stellar systems that appear not to the eye, but only to the mind's eye.

INORGANIC COMBINATION.

Matter is primarily combined by chemical affinity. The sub stances thus produced appear in three states: gaseous, fluid, and solid, but are not clearly demarcated. That chemically combined matter which is found in the solid state is further combined by crystallization and lithifaction. It may be that these methods are parts of the same process, and further, that they are one with chemical affinity; at any rate it is impossible clearly to demarcate them. They are also influenced by gravity, and to a large extent act under its control. Thus it is that gravity, and affinity with its concomitants, unite in molecularly combining matter into inorganic substances. Again, these bodies are mechanically combined into geologic formations, bodies of water, and bodies of air, and such combinations result from gravity. Finally they are all combined into an aggregate, the earth itself, solid, fluid, and gaseous. This also results from gravity.

In the succession of combinations thus briefly reviewed, the first natural aggregate reached is the earth. Below that we have chemical and mechanical substances, which do not constitute integers, but only integral parts. The earth itself is a whole—an aggregation, as the term is here used.

Again, the earth is one of the bodies of the solar system, which is a combination of worlds. This aggregation, also, is controlled by gravity. Other higher astronomic aggregates may exist.

ORGANIC COMBINATION.

Portions of the matter combined by affinity and gravity are seg-

regated to be combined by vitality, giving organic bodies or aggregates, as plants and animals. These bodies do not permanently remain such, as the matter of which they are composed sooner or later returns to the condition of combination due solely to affinity and gravity. They live and die.

SUPERORGANIC COMBINATION.

There are certain biotic bodies whose activities are combined. The first step in combination is the biologic differentiation of the sexes, giving a group of co-operative individuals for the activities of reproduction—male and female, parent and child. This initial combination is crudely developed into still larger combinations of co-operative individuals among the lower animals. With mankind it is developed to a much higher degree, resulting in a great variety of co-operative activities.

There is found, then, a variety of methods of combination, included under three classes: physical, due to affinity and gravity; biotic, due to vital organization; and anthropic, due to related activities. Physical combinations result in the production of *substances* and *aggregates*, and the existence of a physical body is preserved by preserving identity of form and identity of constituent matter. Biotic combination also produces substances and aggregates, and the existence of a biotic body is continued by the preservation of identity of form, but not of identity of constituent matter. In anthropic combination, substances and aggregates, as the terms are here used, are not produced, but biotic aggregates are interrelated in their activities through the agency of mind.

In physical aggregates the relation of parts is that of interdependence, so that the constitution and form of each part are dependent on the constitution and form of every other part. This interdependence may be better comprehended by means of an illustration. In the aggregate the earth, the interdependence is exhibited in the relations existing between the incompletely aggregated bodies of minerals, known as geologic formations; the incompletely aggregated bodies of water, known as seas, lakes, streams, and clouds; and the incompletely aggregated bodies of air, known as winds. Air-currents gather the waters from the seas and pour them upon the lands. Rains and rivers disintegrate the rocks and carry them to the sea. Currents in the sea distribute the detritus over

the bottom. By the loading of areas of sea-bottom they are depressed, and by the degradation of land-areas they are unloaded and rise. Change in the geography of the land effects a change in wind-currents and in bodies of water, and a change in the latter effects a change in sedimentation. In like manner, throughout all physical nature, an interdependence of parts is exhibited. Part acts *on* part.

In biotic aggregates the same interdependence of parts is shown. Any change affecting the digestive apparatus affects the circulatory apparatus, and these again are influenced by the respiratory apparatus. But in addition to this interdependence of parts, there is also an organization of parts—that is, special functions are performed by the several parts, and each is the organ of its function. And this organization is of such a nature that each works for the others. The digestive apparatus digests for itself and all the organs, the heart propels for all the body, the eye sees for all the body, the ear hears for all the body, the hand touches for all the body. Thus the organic parts act *on* and *for* one another.

In activital combination, aggregates, as the term is here used, do not appear, but the same interdependence is observed. By association the sanitary state of the husband affects that of the wife, and the condition of the mother affects the child; and on through the different combinations of animals and men this interdependence is observed. The relation of organization also exists by the differentiation of industries. The husband brings food to the wife and children, and the wife prepares the food. And this differentiation of industries, or "division of labor" as it is termed in political science, is carried on to an elaborate condition in civilized life. Then men are related to one another as constituent members of society; one commands and another obeys. Then men are related to one another through language; one speaks, another hears; one writes, another reads. Then men are related to one another through opinions; having common opinions, they form common designs and act for common purposes. It will thus be seen that superorganic or anthropic combination arises from the establishment of four classes of relations, corresponding to the four classes of activities represented by arts, institutions, languages, and opinions. The arts are human activities directed to the utilization of the materials of nature and the control of its powers, for the purpose of securing happiness. Institutions are human activities arranged for the purpose of securing

peace and establishing justice, and thereby increasing happiness Languages are activities devised for the purpose of communicating thought, and thereby securing happiness. Opinions arise from psychic activities, the purpose of which is to learn the truth, that happiness may ensue.

In physical, biotic, and anthropic combinations the parts control one another. It will therefore be convenient to speak of three kingdoms of matter: the mineral or physical kingdom, the organic or biotic kingdom, and the anthropic or activital kingdom.

MODES OF MOTION.

All bodies, however combined, are discovered to be in motion.

Among the bodies of the mineral kingdom, a variety of modes of molecular motion are exhibited, having various distinguishing characteristics. These are heat and light, electricity and magnetism, then sound and that motion in gases by which through impact they retain their rarefied state. Again, a variety of molar motions are observed in gases, liquids, and solids; and finally stellar motions are observed in astronomic systems.

In the biotic kingdom plants and animals exhibit many varieties of organic motions, called *functions.* These are superadded to the physical motions, which appear alike in the physical and biotic kingdoms. Physical bodies exhibit motions; biotic bodies exhibit motions and functions, the latter being highly organized motions.

In the anthropic kingdom there is a complexity of motions arising from biotic functions, which are arranged and combined so as to produce *activities.* These activities are represented by arts, institutions, languages, and opinions.

Thus there are three great classes of motions corresponding to the three great classes of combinations, namely, physical motions; biotic motions, or functions; and anthropic motions, or activities.

THE RELATION OF MOTION TO COMBINATION.

It will at once be seen that anthropic combination is such by virtue of human activities. Activital combination is manifestly composed motion.

Again, biotic aggregates are such by virtue of continuous combination and dissolution. Within proper limits a biotic body may be compared to a river; it is a form through which matter passes. In

plants some of this passing matter becomes fixed for a time, but eventually returns from the biotic to the mineral kingdom. Among animals this passage of physical matter through the biotic form is more rapid. The organic functions, also, of these bodies are but arranged or organized motions. Life is motion—the specific motion called *function*.

Again, among the aggregations of the physical kingdom, stellar systems are aggregates by virtue of motion. The combination observed is due to composed motion. Of the mechanical combinations, that exhibited in the atmosphere is such by virtue of motion—that is, the gaseous state is preserved by the interference of molecular motions, and the bodies into which it is imperfectly differentiated, *i. e.*, currents of air, are such by virtue of motion. Again, the imperfectly aggregated bodies of water are such by virtue of motion. This is seen to be true of the clouds floating in the air, and of rivers rolling to the seas. Lakes with outlets are bodies of water in motion, forever fed from the clouds, forever discharging into the sea; and mediterranean seas without outlet are perpetually receiving and discharging their waters; and so far as the sea is differentiated into currents, these are bodies imperfectly aggregated by motion.

There yet remain certain molecular combinations of inorganic substances, due to affinity and gravity, the nature of which is not so immediately perceived. Now, as all societies and other anthropic combinations are such by virtue of their motions, known as activities, and as all biotic bodies are such by virtue of their functions, and as all stellar combinations are such by virtue of stellar motion, and as finally all mechanical combinations are such by virtue of motion, it is at once suggested as an inductive hypothesis that those combinations the nature of which is yet unknown are also such by virtue of motion. It is an hypothesis worthy of consideration, that affinity and gravity are also due to motion. It has even been supposed by some that chemical and barologic methods of combination are but diverse modes of the same process; that affinity and gravity constitute but one method of combination, and that we call it *affinity* when the combination involves minute bodies, below our sense perceptions, and *gravity* when larger bodies are involved.

An attempt has thus been made to define the three kingdoms of matter in terms of matter and motion, showing that there are three

methods of combination, and that the parts combined are related by three corresponding methods, and that in each kingdom motions of a distinctive class are discovered. The constitution of physical bodies is due to composed motion; the constitution of biotic bodies is due to composed transmutations of motion; anthropic combinations are due to related activities.

In order that there be evolution, there must be change in combination of matter and in mode of motion. The sole property of matter is motion, and motion itself is change of position. But this change of position results in change of combination, and change of combination results in change of mode of motion. These changes must now be set forth.

CHANGE OF COMBINATION.

If the mind could discern and classify all the bodies of the universe at any one moment, only space conditions would enter therein; but bodies change from time to time, so that there are sequences of combination. Substances and aggregates of matter are such by reason of an arrangement in position of their constituent parts. Substances and bodies change in external relations and in internal relations. Change in external relations is change of position in relation to external things. Change in internal relations is the change in relative arrangement of constituent parts. And this change of position is always motion, the first and only property of matter.

Chemical, crystalline, and lithical combinations are decomposed and otherwise re-composed, mechanical combinations are broken up and otherwise re-arranged, and stellar aggregates are believed to have been gradually formed. With physical bodies internal change is the direct result of external change. This is their distinctive characteristic, that all their changes of constitution result directly from agencies without themselves.

Biotic bodies exhibit the same changes as mineral bodies, and also a series peculiar to themselves. First, biotic substances are segregated from the mineral kingdom—*i. e.*, mineral substances are changed into biotic substances. Second, biotic bodies begin, grow, decline, and die. This is a progressive change of structure. Third, the structure of biotic bodies is preserved by continuous change in their constituent matter. Form and structure are preserved while the matter is forever changing. Life is a determined, systematic sequence of transmutations of motion, transformations of matter, and

transfigurations of body. Life is change. Fourth, as the individuals are not persistent, the method of aggregation continues by the processes of reproduction of like forms. But these like forms are made unlike—*i. e.*, changed—by two processes. In the biotic reproduction of the higher forms the bisexual method prevails, so that each individual is the offspring of two parents, like both so far as they are alike, but differing from the one or the other so far as they are unlike. Fifth, the individual has its constitution determined by its parents, but subject to changes which may be brought about by external relations differing from those to which the parents were subjected; and within limits these are transmitted to offspring. Thus it is seen that biotic changes are caused by external and internal agencies.

This may be put in another form. In mineral bodies the same matter is changed in structure. In biotic bodies the same or nearly the same structure remains and the constituent matter changes; yet there is a slow change in structure from birth to death, and a still further change in structure from generation to generation; but there is more rapid change of constituent matter. Anthropic aggregates arise, not by a combination of matter, but by a combination of the activities of biotic bodies. These biotic bodies themselves change, as individuals disappear and new ones take their places. Thus family group succeeds family group, and generations of people succeed generations of people. In the same manner arts change. Old arts are abandoned and new arts appear. Various societies cease to exist and new societies are organized. The organization due to the differentiation of operations steadily increases by the division of labor; and the grouping of bodies of men into states, *i. e.*, tribes and nations, is in constant flux. So, languages change—they grow and die. And opinions change with each individual and from generation to generation. All these changes are determined by the will of the individual units who are actors—that is, activities change because the actors so desire. Anthropic change is due to psychic agencies.

CHANGE OF MOTION.

That motion is persistent is a fundamental axiom. But while it does not change in quantity it changes in quality in diverse ways. First, motion may be changed in direction. Simple motion is the motion of a body in a straight line, and change of such motion of

the lowest order is change in direction, and this is accomplished by the combination of two or more motions having different directions. Then motion may be transmitted from one body to another. The molecular motions—heat, light, electricity, sound, etc.—are motions propagated by transmission from molecule to molecule. In the kinematic hypothesis of gravity it is held that atomic motion is transmitted from atoms to combined and aggregated bodies by impact; and here we reach another method of change—that by transmutation. One mode of motion may be transmuted into another, as molar motion into heat, and heat into electricity.

By the combination of matter motion is composed. Mineral substances and aggregates exhibit this composition of diverse modes of motion. Biotic bodies exhibit composition of modes of motion, and also composition of transmutations of motion, and it is this latter characteristic which distinguishes biotic from physical motion. Activital combinations exhibit a composition of modes of motion, and a composition of the transmutations of motion, and a composition by co-operative action. It is the last characteristic which distinguishes activital motion from biotic.

The changes of motion exhibited in the mineral kingdom are changes in direction by combination, changes in relative quantity by transmission, changes in mode of motion through transmutation, and changes in the combination of modes of motion.

In the biotic kingdom the same changes are found as in the mineral kingdom, but to them are added changes in the composition of transmutations of motion.

In the anthropic kingdom all the changes in the other kingdoms appear, together with changes in the composition of activities.

EVOLUTION DEFINED.

As matter is indestructible, when one combination or aggregation is dissolved some other must appear, and *vice versa.* Existing bodies must have antecedents. In tracing backward the history of bodies, lines of sequences are followed. Many such are known, and the first important characteristic to be noted of them is *they are orderly.* Like bodies have like antecedents. From this results one of the highest inductions of science, namely, that from consequents antecedents can be restored, and from antecedents consequents can be predicted. The second important characteristic of these sequences

of change is that *many are in a definite direction*, which is gradually becoming known. This general course of change is denominated Evolution, and the term must be defined.

Evolution is progress in systemization. It must be noted that not all changes are progressive; some are retrogressive. It is only progressive change that is here called evolution; retrogressive change is dissolution. As the term is here used, a System is an assemblage of interdependent parts, each arranged in subordination to the whole so as to constitute an integer. Evolution may therefore be defined in another way. It is progress in differentiation by the establishment of unlike parts, and in the integration of these parts by the establishment of interdependence. Dissolution is retrogression by the lapsing of integration through the destruction of interdependence, and the lapsing of differentiation through the loss of heterogeneity in parts.

EVOLUTION IN THE PHYSICAL KINGDOM.

Under the kinematic hypothesis, which embraces the ethereal and nebular hypotheses, portions of discrete matter have been segregated to be combined and aggregated. The process precedent to evolution, then, is combination and aggregation, by which substances and integers are produced.

Whatever may be the fate of the explanation of the origin of substances and aggregates through the kinematic and concomitant hypotheses, the fact remains that such bodies exist, and the evolution of matter, as it is hereafter dealt with, starts from this point. Given substances and aggregates as they are known to exist in nature, and given changes which they are known to undergo, it is proposed to point out by what methods evolution is attained.

The terms *substance* and *aggregate* have been used as distinguishing two orders of combination. It should be noted that they cannot be clearly demarcated. Substances are composed of homogeneous, non-interdependent parts, but this homogeneity is never absolute, and some slight degree of interdependence may always be discovered. Aggregates, on the other hand, are composed of heterogeneous, interdependent parts, but degrees of heterogeneity and interdependence appear. Combination is the bringing together of dissociated matter; and it is in the combinations, separations, and re-combinations of matter that evolution appears.

In mineral bodies combinations proceed by molecular, molar, and stellar methods. It has been shown that the changes in these bodies are due to external conditions or forces. If a given body be in harmony with external conditions no change occurs in its constitution, but if it be out of harmony the impinging agencies effect such modifications as will produce harmony. This may be done by a change in the body as a substance or aggregate, or by its separation and re-combination in some more harmonious form. The evolution of mineral bodies is thus accomplished by direct adaptation to external conditions.

If it is permitted hypothetically to conceive of a universe of ethereal matter—*i. e.*, matter composed of discrete atoms in motion, such atoms would remain in an attenuated condition by atomic impact. In matter thus constituted, motion could be transmitted from atom to atom, but no new mode of motion would result therefrom. The mass of matter thus constituted would be absolutely homogeneous. But if by some method several such atoms should be combined, so as to move together as a common body, and so that their interspaces could not be penetrated by other atoms, the motion of an impinging atom would not only be transmitted to the larger body, but it would also be transmuted into another mode or kind of motion. If other such molecules were formed by the segregation of atoms from the homogeneous mass, the new kind of motion would be set up in all the matter thus segregated, and the motions of these bodies would react one upon another. If, again, some of these molecules were segregated, to be combined in larger bodies, with or without such a diminution of interspaces as to prevent the interpenetration of atoms, a third mode of motion would be established; and if diverse methods of aggregation should occur, diverse modes of motion would be established thereby; and in all combining and re-combining, aggregating and re-aggregating, new modes and complexities would arise.

It is a well-known law that a moving body passes in the direction of the least resistance. Diverse modes of motion may exist in a body, due to the complexities of its organization. In the transmission of motion to such a body from another by impact, the motion transmitted is transmuted into that mode which gives it the least resistance. This is illustrated on every hand. When a smaller body impinges against a larger, the inequality between the two may be so great that molar motion is not set up in the

larger body, but the whole of the imparted motion is transmuted into heat or some other molecular motion.

This law, that motion passes in the direction of least resistance, is the equivalent of the law of adaptation in the evolution of matter. When evolution is considered from the standpoint of matter, it is convenient to use the term *Adaptation;* when considered from the standpoint of motion, it is more convenient to use the term *Least Resistance.*

EVOLUTION IN THE BIOTIC KINGDOM.

In biotic bodies it has been seen that change is the result of internal as well as external conditions. As external conditions, or the environment, are changing, these bodies change to a limited extent, in the same manner as do mineral bodies; but there is also a change brought about indirectly by the environment, through certain internal changes in the constitution of biotic bodies. Through this internal constitution individuals are changed in time—one generation dies and another succeeds.

There is yet another method of change in biotic bodies, which steadily increases from the lowest to the highest—that is, the change in their constituent matter. While structure changes slowly from birth through growth and decadence to death, the constituent matter changes with much greater rapidity. In this change the minute elements of structure change much more rapidly than the larger into which they are compounded; so that every part of the organ must be supplied with new material to replace that which is steadily becoming effete and passing away. Now the rate of this change in any integral part of an organism is dependent upon the activity of the organ. Exercise increases the rate of change in the constituent matter of a biotic organ, and thus the slow change in its structure, which proceeds from life to death, is accelerated. This accelerated change results in increased differentiation of the organ, and it thereby becomes more and more efficient in the performance of its function. This change, therefore, results from exercise. Organs that are exercised increase in efficiency, by non-exercise they decrease in efficiency. This change in the organization of any one individual is but slight, but as the slight changes pass from one generation to another, continuous exercise of one set of organs greatly modifies them; continuous neglect of exercise in another set modifies them also, until at last they are atrophied. Thus by exercise and non-

exercise important structural changes are produced when conjoined with the changes due to heredity.

All these changes result in progress, from the fact that those individuals whose change is in a direction out of harmony with the environment ultimately perish, while those whose change is in a direction in harmony with the environment survive. This method of adaptation or evolution in biology is called "the survival of the fittest."

The rate of evolution by survival is greatly accelerated by another condition. Each pair of biotic bodies reproduce a large number of new bodies, so that reproduction from generation to generation is in a high geometric ratio. The earth having become occupied with all the biotic beings that can derive sustentation therefrom, but a small fraction of the beings produced in a generation can live. Few survive, many succumb. Survival by adaptation is therefore made more efficient by competition.

There are other changes in the biotic kingdom brought about by adaptation. The multiplicity of biotic beings, causing over-population, has crowded them into every conceivable habitat—in the air, on the land, and in the water; and living beings have become adapted thereto by the development of wings, legs, fins, and correlative organs. Thus by exercise organs have been developed, and by non-exercise other organs have been atrophied, until living beings have become specialized for a vast diversity of habitats—for life on the mountain and in the valley, in the light and in the dark, in the cold and in the heat, in humid regions and in arid regions. Living beings have also been adapted to various kinds of food and to various methods of acquisition—in fine, to a great variety of conditions.

This specialization by development, through exercise and non-exercise, must be clearly distinguished from the processes of evolution. The heterogeneous living beings thus produced are but multiplied and diverse forms, animals and plants alike being as often degraded as evolved in the processes of specialization. Degradation is especially to be noticed in parasitic animals and others adapted to extremely abnormal habitats; but it should be understood that a form thus produced may, in the process of its production and subsequent existence, make progressive change in the system of its structure by the methods of evolution already characterized.

Specialization is greatly accelerated by a peculiar method. As

all the higher animals are physically discrete, psychic relations must be established, in order that they may meet for the act of reproduction. These psychic relations gradually develop into choice, or sexual selection, and by methods which have been clearly pointed out by biologists the minute increments of change that result therefrom eventually accumulate into strong variations, always adapted to the conditions of the environment. Thus the survival of the fittest is accelerated by sexual selection.

EVOLUTION IN THE ANTHROPIC KINGDOM.

If attention is directed exclusively to animal life, we notice that evolution has proceeded *pari passu* with specialization. Of the forms that have been specialized from time to time some have become extinct, some have been degraded, and some have been evolved in varying degree. One form, not the most specialized, made the greatest progress in evolution, until an organism was developed of so high a grade that this species became more independent of environment than any other, and, by reason of its superiority, spread widely throughout the land portion of the globe This superior animal was early man, when he first inhabited all the continents and the great islands. The production of this superior, *i. e.* more highly systematized organism, was the antecedent to the inauguration of new methods of evolution.

It has been shown that the great efficiency of the biotic method of evolution by survival depends upon competition for existence in enormously overcrowded population. Man, having acquired superiority to other animals, passed beyond the stage when he had to compete with them for existence upon the earth and into the stage where he could utilize plants and animals alike for his own purposes. They could no longer crowd him out, and to that extent the law of the survival of the fittest in the struggle for existence was annulled in its application to man. He artificially multiplies such of the lower animals as are most useful to him, and domesticates them, that they may be more thoroughly under his control, and modifies them, that they may be more useful, and uses such as he will for beasts of burden; and the wild beasts he destroys from the face of the earth. In like manner he cultivates useful plants, and destroys such as are worthless to him. He does not compete with other biotic species, but utilizes them for his welfare. Yet

the law of the survival of the fittest applies in so far as it is not dependent upon competition, and slow evolution may still result therefrom. But at this stage new methods spring up of such great efficiency that the method by the survival of the fittest may be neglected because of its insignificance.

In anthropic combinations the units are men, and men at this stage are no longer passive objects, but active subjects; and instead of man being passively adapted to the environment, he adapts the environment to himself through his activities. This is the essential characteristic of anthropic evolution. Adaptation becomes active instead of passive. In this change certain parts of the human organism are increasingly exercised from generation to generation. This steadily increasing exercise results in steadily increasing development, and the progress of the unit—man—in this higher organization depends upon development through exercise. But the progress by exercise depends upon the evolution of activities.

Man is an animal, and may be studied as such; and this branch of science belongs to biology. But man is more than an animal. Though an animal in biotic function, he is man in his anthropic activities; for by them men are combined—*i. e.*, interrelated—so that they are not discrete beings, but each acts on, for, and with, his fellow-man in the pursuit of happiness. Human activities, thus combined and organized, transcend the activities of the lower animals to such a degree as to produce a new kingdom of matter. The nature of these activities must here be set forth.

The first grand class is composed of those which affect the external world, and by them men are interrelated through their desires. These activities are the Arts. The arts have been evolved by human invention, and man has been impelled thereto by his endeavor to supply his wants. In the course of the evolution of the arts, man has progressively obtained control over the materials and powers of nature. All the arts of all the human period are the inventions of men. But invention has proceeded by minute increments of growth. A vast multiplicity of arts have been devised, of which comparatively few survive in the highest civilization. As the inventions have been made, the best in the average has been chosen. Man has therefore exercised choice. The evolution of the arts has thus been by the method of invention and choice, in the endeavor to gratify desire, and by them man has adapted the environment to himself.

Second. There is a grand class of activities through which men

are interrelated in respect to their conduct. These activities result in Institutions. Through them men are associated for a variety of purposes. Every institution is an organization of a number of individuals, who work together for a common purpose, as, for example, to prosecute some industrial enterprise, to co-operate in the pursuit of pleasure, to promote some system of opinions, or to worship together under the forms of some religion. All such institutions constitute a class denominated Operative Institutions. A second class are the institutions which man has organized for the direct regulation of conduct. These are States and their subordinate units, with their special organs of government, and rules for the regulation of conduct, called Laws.

Institutions have been developed from extreme simplicity to extreme complexity. They are all the inventions of mankind, and their evolution has been by minute increments of growth. Their invention has been wrought out that men might live together in peace and render one another assistance; and gradually, by the consideration of particulars of conduct as they have arisen from time to time, men have sought to establish justice, that they might thereby secure peace. Of the vast multiplicity of institutions—forms of state, forms of government, and provisions of law—which have been invented, but few remain in the highest civilization, and these few have been selected by men. Men have thus exercised choice. Institutions, therefore, have been developed by invention and the choice of the just in the endeavor to secure peace.

Third. There is another fundamental group of activities through which men are interrelated in respect to their thoughts. These are the activities of mental intercommunication, and result in Languages. Languages, also, are inventions by minute increments of growth. Many languages have been invented, and in each language many words and many methods of combining linguistic devices have been invented. In the languages of the most civilized peoples, but few of these survive; and there are spoken by all the peoples of the earth but few languages in comparison to the many that existed in the early history of mankind; and the method of survival, when analyzed, is found also to be choice. Men have chosen the economic in the expression of thought. Languages, therefore, have developed by invention and choice in the struggle for expression.

Fourth. There is a grand class of activities by which men are interrelated in respect to their designs. Men arrive at Opinions, and

these have always reacted upon languages, institutions, and arts, and largely led them in their courses of progress. Because of their opinions, men are willing to work together, and thus have common designs. There have been many opinions and many systems of philosophy. Of all that have existed, but few remain in the highest civilization. A careful analysis of the facts relating to the growth of opinions reveals this truth, that opinions also are invented, and that the final survival of the few has been due to the human act of choice in the selection of the truth. Opinions, therefore, have been developed by invention and choice in the struggle to know.

Fifth. Opinions are formed as the direct activities of the Mind. Languages, institutions, and arts have arisen through the action of the mind and the exercise of other corporeal functions. All these activities, therefore, are dependent upon the mind. On the other hand, these objective activities react upon the mind, so that mental operations are controlled thereby. Through the exercise of the mind in the prosecution of activities it is developed. These mental activities are perception and comparison, or reflection, as it is more usually called. The subjective evolution of the mind is therefore the product of the objective evolution of activities.

These five great classes of activities are interdependent in such a manner that one is not possible without the others; they arise together, and their history proceeds by a constant interchange of effects. All the five classes of activities react upon man as an animal in such a manner that his biotic history subsequent to his differentiation from the lower animals is chiefly dependent thereon. The evolution of man as a being superior to the beast is therefore due to the organization of activities.

It has been shown that man does not compete with the lower animals for existence. In like manner, man does not compete with man for existence; for by the development of activities men are interdependent in such a manner that the welfare of one depends upon the welfare of others; and as men discover that welfare must necessarily be mutual, egoism is transmutted into altruism, and moral sentiments are developed which become the guiding principles of mankind. So morality repeals the law of the survival of the fittest in the struggle for existence, and man is thus immeasurably superior to the beast. In animal evolution many are sacrificed for the benefit of the few. Among mankind the welfare of one depends upon the welfare of all, because interdependence has been established.

It has thus been shown that there are three stages in the combination of matter and motion, and that each stage is characterized by a clearly distinct method of evolution. These may be defined as follows:

First, physical evolution is the result of direct adaptation to environment, under the law that motion is in the direction of least resistance.

Second, biotic evolution is the result of indirect adaptation to the environment by the survival of the fittest in the struggle for existence.

Third, anthropic evolution is the result of the exercise of human faculties in activities designed to increase happiness, and through which the environment is adapted to man.

These may be briefly denominated: evolution by adaptation, evolution by survival of the fittest, and evolution by endeavor.

Civilized men have always recognized to some extent the laws of human evolution,—that activities are teleologically developed, and that happiness is increased thereby. In the early history of mankind the nature of teleologic endeavor was so strongly impressed upon the mind that the theory was carried far beyond the truth, so that all biotic function and physical motion were interpreted as teleologic activity. When this error was discovered, and the laws of physical and biotic evolution established, vast realms of phenomena were found to have been entirely misunderstood and falsely explained, and teleologic postulates have finally fallen into disrepute. Men say there is progress in the universe by reason of the very laws of nature, and we must let them alone. Thus, reaction from the ancient false philosophy of teleology has carried men beyond the truth, until they have lost faith in all human endeavor; and they teach the doctrine that man can do nothing for himself, that he owes what he is to physical and biotic agencies, and that his interests are committed to powers over which he has no control.

Such a philosophy is gradually gaining ground among thinkers and writers, and should it prevail to such an extent as to control the actions of mankind, modern civilization would lapse into a condition no whit superior to that of the millions of India, who for many centuries have been buried in the metaphysical speculations of the philosophy of ontology. When man loses faith in himself, and worships nature, and subjects himself to the government of the

laws of physical nature, he lapses into stagnation, where mental and moral miasm is bred. All that makes man superior to the beast is the result of his own endeavor to secure happiness.

Man, so far as he is superior to the beast, is the master of his own destiny, and not the creature of the environment. He adapts the natural environment to his wants, and thus creates an environment for himself. Thus it is that we do not discover a biotically aquatic variety of man, yet he dwells upon the sea and derives sustentation from the animals thereof by means of his arts. A biotically arboreal variety of man is not discovered, but the forest are used in his arts and the fruits of the forests for his sustentation. An aërial variety of man is not discovered, but he uses the winds to propel his machinery and to drive his sails; and, indeed, he can ride upon the air with wings of his own invention. A boreal variety of man is not discovered, but he can dwell among the everlasting snows by providing architectural shelter, artificial warmth and bodily protection.

Under the influences of the desert a few plants secure a constitution by which the moisture imbibed during brief and intermittent rains is not evaporated; they become incrusted with a non-porous glaze, or contract themselves into the smallest space and exist without life until the rain comes again. Man lives in the desert by guiding a river thereon and fertilizing the sands with its waters, and the desert is covered with fields and gardens and homes. Everywhere he rises superior to physical nature. The angry sea may not lash him with its waves, for on the billows he builds a palace, and journeys from land to land. When the storm rises it is signaled from afar, and he gathers his loved ones under the shelter of his home, and they listen to the melody of the rain upon the roof. When the winds of winter blow he kindles fossil sunshine on his hearth, and sings the song of the Ingleside. When night covers the earth with darkness he illumines his path with lightning light. For disease he discovers antidote, for pain nepenthe, and he gains health and long life by sanitation; and ever is he utilizing the materials of nature, and ever controlling its powers. By his arts, institutions, languages, and philosophies he has organized a new kingdom of matter, over which he rules. The beasts of the field, the birds of the air, the denizens of the waters, the winds, the waves, the rivers, the seas, the mountains, the valleys, are his subjects; the powers of nature are his servants, and the granite earth his throne.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

GENERAL MEETING.

BULLETIN

OF THE

GENERAL MEETING.

227TH MEETING. JANUARY 13, 1883.

The President in the Chair.

Twenty-six members present.

Mr. H. FARQUHAR completed a communication begun at the 224th meeting on

EXPERIMENTS IN BINARY ARITHMETIC,

in which he showed that simple addition involved carrying on several distinct mental operations almost simultaneously and a capital of more than fifty propositions committed to memory. Believing that the difficulty in mastering, and the mental strain and liability to error in conducting, this most important of mathematical processes could be proved to be unnecessarily great, he had compared the time occupied in adding a few dozen numbers of six or eight figures each with that required when these numbers were expressed in powers of 2, the mental work being, in the latter case, reduced to counting similar marks and halving their sums. He had found it best to give different forms to the marks denoting neighboring powers, so as to avoid confusion of columns, and had combined two or more of them into one written figure for brevity of expression. About seventy combinations of various shapes had been tried, but very few of them found economical. In the best notation, however, the addition required only three-fourths the time taken with the ordinary figures. Had the computer practised as many weeks with the new notation as years with the old, the difference would have been much more marked; as it was in fact when one unskilled in arithmetic, to whom the binary notation had just been taught, tried the two additions. The gain in accuracy, with this observer, was even

more striking than the gain in speed. There could be very little doubt, therefore, that a fair degree of skill in arithmetic with a binary notation could be acquired by many to whom it is impossible under the present system.

The only practicable division of arcs and angles, and the most natural division of all things, is by continued bisections. This is shown by the ratio of value in our coins, weights, and capacity measures; by any table of prices; and by the prevalent subdivision of lowest nominal units, as of the carpenter's inch into eighths and sixteenths, and of percentages into quarters, etc., in stock quotations, where convenience of calculation by our present arithmetic seems almost gratuitously sacrificed. The American coinage is inconvenient in practice, because of the awkward fractional ratio $2\frac{1}{2}$, which it introduces between successive pieces; and there would be the same difficulty in a decimal system of weights or of measures, should it be imposed upon us. We have thus another powerful reason for endeavoring to introduce a binary arithmetic.

In the remarks which followed, Mr. E. B. ELLIOTT expressed the hope that Congress would adopt the metric system of weights and measures for international purposes. It would be better to secure what advantage could be gained from uniformity and consistency, even though the basis of consistency was an arithmetic not ideally the best attainable. Such a course would not prevent, but might pave the way for a better arithmetic.

Mr. W. B. TAYLOR said the world was losing so much by the employment of the denary arithmetic that he thought even a single generation might find economy in substituting the octonary. The introduction of decimal measures, while it would aid the computer, would injure the remainder of the community. The paper of Mr. Farquhar had an especial value, in that it proved the ability of binary systems to compete with the established system in rapidity of computation.

Other remarks were made by Messrs. HARKNESS, MUSSEY, POWELL, and GILBERT.

The next communication was by Mr. S. M. BURNETT on

REFRACTION IN THE PRINCIPAL MERIDIANS OF A TRIAXIAL ELLIPSOID; REGULAR ASTIGMATISM AND CYLINDRICAL LENSES;

and he was followed by Mr. W. HARKNESS on

THE MONOCHROMATIC ABERRATION OF THE HUMAN EYE IN APHAKIA.

These two papers are complementary, and are published in the *Archives of Ophthalmology*, Vol. XII, No. 1.

228TH MEETING. JANUARY 27, 1883.

The President in the Chair.

Thirty-seven members present.

The Auditing Committee, appointed at the Annual Meeting, reported through its chairman, Mr. Antisell, that it had examined the accounts of the Treasurer for 1882, and found them correct.

The report was accepted.

The communication of the evening was by Mr. H. H. BATES on

THE NATURE OF MATTER,

and was discussed by Mr. W. B. TAYLOR and Mr. POWELL.

This paper is published in the Popular Science Monthly for April, 1883.

229TH MEETING. FEBRUARY 10, 1883.

The President in the Chair.

Forty-two members and visitors present.

It was announced that reports of the scientific proceedings would hereafter be furnished to Science.

Mr. W. H. DALL announced that an opportunity would be afforded members to contribute to the Balfour Memorial Fund.

A communication was then read by Mr. A. F. A. KING on

THE PREVENTION OF MALARIAL DISEASES, ILLUSTRATING, *inter alia*, THE CONSERVATIVE FUNCTION OF AGUE.

[Abstract.]

The various theories thus far presented in explanation of the

phenomena of malaria were unsatisfactory and insusceptible of scientific demonstration.

According to the best medical authorities the most generally admitted facts upon which the present orthodox theory of malaria rests were as follows: 1. Malaria affects by preference low and moist localities. 2. It is almost never developed at a lower temperature than 60° F. 3. Its evolution or active agency is checked by a temperature of 32° F. 4. It is most abundant and most virulent as we approach the equator and the sea-coast. 5. It has an affinity for dense foliage, which has the power of accumulating it, when lying in the course of winds blowing from malarious localities. 6. Forests or even woods have the power of obstructing and preventing its transmission under these circumstances. 7. By atmospheric currents it is capable of being transported to considerable distances—probably as far as five miles. 8. It may be developed in previously healthy places by turning up of the soil, as in making excavations for the foundations of houses, tracks for railroads, and beds for canals. 9. In certain countries it seems to be attracted and absorbed by bodies of water lying in the course of such winds as waft it from the miasmatic source. 10. Experience alone can enable us to decide as to the presence or absence of malaria in any given locality. 11. In proportion as countries, previously malarious, are cleared up and thickly settled, periodical fevers disappear, in many instances to be replaced by typhoid fever (?) 12. Malaria usually keeps near the surface of the earth. It is said to "hug the ground," or "love the ground." 13. It is most dangerous when the sun is down, and seems almost inert during the day. 14. The danger of exposure after sunset is greatly increased by the person exposed *sleeping* in the night air. 15. Of all human races the white is most sensitive to marsh fevers, the black least so. 16. In malarial districts the use of fire, both indoors and to those who sleep out, affords a comparative security against malarial disease. 17. The air of cities in some way renders the poison innocuous; for, though a malarial disease may be raging outside, it does not penetrate far into their interior. 18. Malarial diseases are most prevalent towards the latter part of summer and in the autumn. 19. Malaria is arrested not only by trees, but also by walls, fences, hills, rows of houses, canvas curtains, gauze veils, mosquito nets, and probably by fishing nets. 20. Malaria spares no age, but it affects infants much less frequently than adults.

These generally admitted facts were insusceptible of scientific explanation by the marsh fever hypothesis of Lanscisci; but were capable of explanation by the theory that marsh fevers are produced by the bites of proboscidian insects, notably in this and in some other countries by mosquito bites.

A review of the natural history, habits, and geographical distribution of the mosquito was next presented in explanation of the twenty statements above quoted.

In discussing statement 15, it was maintained that the comparative immunity of the black races was largely due to color, the dark complexion of the skin being another illustrative instance of "protective coloring" so often observed in other animals, and by which, in this instance, the negro was protected from the *sight*, and consequently from the *bite* of the mosquito; a similar protection being further secured by the offensive odor and greasiness of his cutaneous secretions, aided by artificial inunction of the body with grease, paint, pitch, &c., which last probably constituted the initial step in the evolution of dress. Hence malarial melanosis was considered to be the designed natural termination of ague—its conservative function—destined to modify the individual by defensive adaptation against the mosquito, whose penetrating proboscis, like an inoculating needle, infected the body with malarial poison, no matter whether this last was mosquital saliva, the *Bacillus malariæ* of Klebs and Crudelli, or some other element as yet unknown.

The spleen, whose function is not yet settled by physiologists, was regarded as the chief *pigment-forming organ*, and was *designed for this purpose in the economy of the organism.* Generally considered a superfluous organ, capable of removal without any great interference with the functions of the organism, it was naturally designed to *meet the emergency of variation in skin-color* to secure "protective coloring" against fever-producing proboscidian insects, as before indicated. The natural process, however, required exposure of the naked body to the sun during the chill stage, in order to secure deposit of the newly formed pigment in the skin. Nature had not anticipated the artificial appendage of dress, and the organism had not inherited from ancestral progenitors any provision for so unexpected an addition. Chills do not occur at night, but only between the rise and setting of the sun; sunlight during the chill stage being a necessary requirement, in order that nature's design of cutaneous chromatogenesis may be consummated. Other racial

differences between the whites and blacks—such as even cerebral capacity and variations in the skeleton—might be susceptible of explanation by blood changes resulting from malaria. The marrow of bones was also a pigment-forming tissue, and the aching of bones during ague, especially in so-called "break-bone" fever, suggested congestion and modified nutrition in the osseous structures, such as might eventually lead to modification in the skeleton. The inhabitants of oriental countries especially were more vigorous and intelligent if they lived in elevated regions, than were others inhabiting mosquito-infected lowlands and sea coasts.

In further support of the mosquital origin of malarial fevers numerous noted medical authorities were cited, showing that, in all parts of the world where these diseases prevail, immunity was secured by protecting the body from mosquito bites. The geographical distribution and seasonal evolution of mosquitoes and other proboscidian insects were shown partially to agree with the times and places in which malarial diseases prevail; though from lack of information conclusive evidence on this point was yet wanting. There was, however, a general admission on the part of medical authorities that swarms of these insects in almost any locality were a pretty sure sign of malignancy.

On the other hand numerous instances were adduced from "Narratives" and "Travels" in which the bodies of persons had been covered with pustules, "resembling small-pox," from mosquito bites without any subsequent occurrence of fever having been *recorded* by the narrating authors.

This opposing evidence was inconclusive, (1) because the authors cited were not in search of medical information; (2) because the period of incubation, being often long and uncertain, fever *may* have occurred after the mosquito bites had been forgotten; (3) the insect proboscis (like a vaccine lancet unarmed with virus) might be uncontaminated with fever poison, or fever germs; and (4) *successful* inoculations of specific germ poisons are *not* usually followed by immediate local suppuration at the point of puncture, but only after a certain period of incubation, the *immediate* local inflammation being rather *preventive* of subsequent blood infection.

The possible spread of yellow-fever contagion by the inoculating proboscis of the mosquito carrying infecting matter drawn from the blood of yellow-fever patients to unaffected persons was suggested. In epidemics, the spread of the disease stopped as soon as a freezing temperature paralyzed the mosquito, &c.

The spread of spotted-fever, typhus-fever, in jails, ships, &c., was referred to the inoculating instrument of fleas, &c.—these insects usually prevailing among filthy people thickly crowded together.

That malarial diseases were ever produced solely by the *inhalation* of supposed poisonous vapors was held to be untenable. Experimenters, who had demonstrated the existence of specific poisons for special fevers, had equally proven that the mode by which such poisons, when obtained, could be introduced into the body for the artificial production of disease, was by *inoculation through the skin.* These experiments were imitations of insect inoculation. The proboscis of the mosquito was Nature's inoculating needle.

The *modus operandi* of the eucalyptus tree in preventing malarial diseases was ascribed tentatively to the tree being destructive to, or interfering directly or indirectly with, the propagation and development of mosquitoes.

From the foregoing conceptions as to the origin of malarial disease, the following prophylactic measures were deducible:

1st. *Personal* protection from all winged insects, especially during evening and night, by gauze curtains, veils, window-blinds, or clothing impenetrable by the proboscis of inoculating insects; and further, personal protection both from these and all creeping insects, especially during epidemics, endemics, and in crowded jails, ships, &c., by daily inunction of the whole body with some terebinthinate, camphorated, or eucalyptalized ointment or liniment.

2d. *Domiciliary* protection (*a*) *exteriorly*, by screens of trees, walls, fences, &c., interposed at some distance between dwellings and the supposed sources of malaria, or mosquito nurseries; and with fires or lamps arranged as traps for the attraction and destruction of such winged insects as may encroach nearer. A further protection (*b*) in the *interior* of dwellings being secured by the use of smoke (as of tobacco or prethrum) or of some volatile aromatic substance, as of camphor, assafœtida, garlic, &c., which may be offensive to proboscidian intruders.

3. *Municipal* protection by groves of trees (pines, cedars, or eucalyptus) planted between cities and the sources of malaria and mosquitoes, together with cordons of electric or other lights, between said grove and the marsh, the lights to be arranged as fly-traps for the retention and destruction of such winged insects as may be thus secured.

With relation to the city of Washington, it was suggested that the Washington monument would afford a good opportunity (by placing illuminated fly-traps at different elevations on its exterior) for ascertaining the height at which mosquitoes fly, or are brought by the wind from the adjacent Potomac flats. The proposed reclamation of the flats could scarcely do more than mitigate malarial disease, so long as our summer and autumn southern breezes come, laden with mosquitoes, from the miles of unreclaimed swamps farther down the river, as at Four-mile Run and other nearer localities.

Mr. BILLINGS remarked that, since ague did not invariably result from insect bites, the most that could be claimed was that they accomplished an accidental inoculation with malarial poison.

The subject was also discussed by Messrs. DOOLITTLE, TONER, and ANTISELL.

The meeting closed with an exhibition by Mr. C. E. DUTTON of a series of oil paintings illustrative of the Hawaiian Islands.

230TH MEETING. FEBRUARY 24, 1883.

Vice-President BILLINGS in the Chair.

Thirty members and visitors present.

The Chair announced the election of Mr. THOMAS RUSSELL to membership.

The first communication was by Mr. G. K. GILBERT on

THE RESPONSE OF TERRESTRIAL CLIMATE TO SECULAR VARIATIONS IN SOLAR RADIATION.

[Abstract.]

Secular variations of climate may theoretically be caused (1) by the internal heat of the earth and (2) by changes in the constitution or volume of the atmosphere. They have unquestionably been wrought (3) by changes in the limits and configuration of ocean bottoms and land surfaces, (4) by changes in the movements of the earth with reference to celestial bodies, and (5) by variations of

solar radiation. Attention will here be restricted to the last-mentioned cause.

An augmentation of the strength of solar radiation (*a*) will cause a general rise in the temperature of the atmosphere, (*b*) will heighten the contrast between warm and cold regions, thereby stimulating oceanic and atmospheric circulation, and (*c*) will heighten the contrast between wet and dry regions, making the wet wetter and the dry drier. (*d*) It will also diminish glaciation. This has been disputed by some writers, but is sustained by a quantitative discussion. A computation, based on the annual curves of precipitation and temperature at St. Bernard, close to the glaciers of the Alps, shows that a general rise in the temperature of the air, while it will increase the total precipitation, will slightly diminish the snow-fall; that it will very greatly increase the rate of melting. The ratio of snow-fall to evaporation is reduced one-half by 6° C rise of temperature; the ratio of snow-fall to melting is reduced one-half by a rise of 1½°; and, assuming that evaporation actually dissipates twice as much snow as does melting, the ratio of snow-fall to snow dissipation (or the tendency to glaciation) is reduced one-half by 4½° rise of temperature.*

(*e*) Increase of solar radiation will also, through its general effects, influence the distribution of winds, and thus produce secondary effects of a local nature.

Mr. DALL remarked that ice was rendered more plastic and fluent by the presence of water; so that the movement of ice and the consequent extent of glaciers are favored by rain. If Mr. Gilbert by the term "glaciation" referred to the *extent* of glaciers, some limitation of his conclusions might be necessary.

Other remarks were made by Messrs. ANTISELL, DOOLITTLE, H. FARQUHAR, and ELLIOTT.

The next communication was by Mr. J. W. CHICKERING on

THE THERMAL BELTS OF NORTH CAROLINA.

[Abstract.]

In the agricultural volume of the Patent Office Report for 1861 is an article written by Mr. Silas McDowell, of Franklin, Macon county, N. C., bearing this title. He was a man of much intelli-

* The computation is given in full in "Science" for March 16, 1883.

gence, an enthusiastic student in geology and botany, a companion and guide of several botanists in their early explorations of the southern Appalachians, and a farmer by profession. He died in 1882, at the ripe old age of 87.

He states that in the valley of the Little Tennessee river, in Macon county, lying about 2,000 feet above tide water, when the thermometer in the morning indicates a temperature of about 26°, the frost line extends about 300 feet in vertical height, but that then comes a belt extending about 400 feet in vertical height up the mountain side, within which no frost is seen, delicate plants remaining untouched. Above this, frost again appears. So sharp is the dividing line that sometimes one-half of a shrub may be frost killed, while the other half is unaffected.

A small river, having its source in a high plateau 1,900 feet above this, runs down into this valley, breaking through three mountain barriers, and consequently making three short valleys, including the plateau, rising one above the other, each of which has its own vernal zone, traversing the hillsides that enclose it, and each beginning at a lesser elevation above the valley, as the valleys mount higher in the atmosphere, so that around the plateau, a beautiful level height, containing 6,000 acres of land, and lying 3,900 feet above tide water, the lower edge of the thermal belt is not more than 100 feet above the common level of the plateau.

Not only does vegetation within this zone remain untouched by frost, so that the Isabella, the most tender of all the native grapes, has not failed to produce abundant crops in twenty-six consecutive years, but mildew, blight, and rust, which often attack vines in the lower valleys, are here unknown, while the same purity and dryness of the air which favor the grape, make this a refuge for the consumptive, as diseases of the lungs have never been known to originate among the inhabitants.

Mr. McDowell adds: "The thermal belt must exist in all countries that are traversed by high mountains and deep valleys, and the only reason why its visible manifestations are peculiar to our southern Alleghanies, is the fact that their precocious spring vegetation is sometimes killed by frost, while the same thing does not happen in the mountains further north."

These statements are corroborated by similar testimony respecting another such belt along the Tryon mountain range in Polk county, N. C.; the specific claim being that such a belt is found

for eight miles in length, extending from 1,200 feet to 2,200 feet above tide water, within which the leaves of plants, shrubs, and flowers remain untouched by frost until the latter part of December, and after a snow storm not a particle of snow remains within the belt, while the tops and sides of the mountains above and the valleys below will be covered.

The verification of these alleged facts would be matters of interest in their economical and sanitary aspects, and would supply data for some interesting researches respecting the nocturnal stratification of the atmosphere.

It is earnestly to be hoped that at some time we may have reliable and continuous thermometrical observations at these and similar stations, to determine the existence, extent, and temperature of such belts.

Remarks were made on this communication by Mr. ALVORD.

Mr. C. E. DUTTON then made a communication on the

GEOLOGY OF THE HAWAIIAN ISLANDS.

[Abstract.]

On the slopes of Mauna Loa are sea beaches, terraces, coral sands, and other evidences of shore action at various levels. The highest that can be positively announced has an altitude of 2,800 feet above the ocean. It can be traced a large part of the way around the island, being discernible even when covered by more recent lava. It does not now lie horizontal, but descends from 2,800 to 400 feet, while on the adjoining island, Maui, there is evidence of submergence. On the farther (western) side of Maui, and on other islands beyond, there is again evidence of upheaval.

All the lavas of the islands are basaltic. Those of Mauna Loa and Kilauea are abnormally basic and are related to certain lavas of New Zealand, called by Mr. Judd "ultra-basalts." The New Zealand rock consists chiefly of olivine; that of Mauna Loa is sometimes more than half olivine, and contains much magnetite and hematite. A Greenland lava, classed also as ultra-basalt, contains the only known native iron of telluric origin. As this suggests the iron meteorites, so the basalts of New Zealand and Mauna Loa suggest the stony meteorites.

The volume of the eruptions of Mauna Loa is enormous; that of 1855 would nearly build Vesuvius, and two of prehistoric date

were greater still. The lava has a high liquidity and flows forty to fifty-five miles, spreading at the base of the cone into a broad sheet. There are no explosive phenomena and no fragmental products. The slope of the mountain is 4° along the major and 7° along the minor axis. Kilauea has a few cinder cones on its flanks. Mauna Kea consists chiefly of them, and has an average slope of 7½° to 11°.

Kilauea is always active, maintaining lakes of liquid fire. Over one of these a crust is formed, black, but flexible, which after a while breaks up and suddenly sinks, the process being repeated at intervals of 1½ to 2¼ hours. The great interior pit described by observers from 1823 to 1841 is now filled.

Mauna Loa is not active more than one-third or one-fourth of the time, but compensates by the magnificence of its phenomena. Great fountains of lava are projected hundreds of feet into the air.

Mr. Dutton's communication was interrupted by the arrival of the hour for adjournment. In response to a question by Mr. TAYLOR, he stated that the crust over a lava lake acquired a thickness of five or six inches before breaking up.

Mr. ANTISELL inquired whether there is any basalt on the islands, and Mr. Dutton explained that they are composed exclusively of that material.

231ST MEETING. MARCH 10, 1883.

Vice-President WELLING in the Chair.

Thirty-four members and visitors present.

The Chair announced that Messrs. ALBERT WILLIAMS, Jr., JOHN HENRY RENSHAWE, and HENRY FRANCIS WALLING had been elected to membership.

Mr. M. H. DOOLITTLE read a communication on

SUBSTANCE, MATTER, MOTION, AND FORCE,

which was discussed by Messrs. W. B. TAYLOR, ELLIOTT, HARKNESS, and WELLING.

Mr. E. B. ELLIOTT then communicated

FORMULAS FOR THE COMPUTATION OF EASTER.

In the calendar the vernal equinox is considered as invariably occurring on the 21st of March.

The Paschal full moon is the full moon which (according to the calendar) occurs on or first after the 21st of March.

Easter Sunday in any year is the first Sunday which occurs *after* the Paschal full moon; that is, first after the full moon which, according to the calendar, occurs *on or first after* March 21st.

To find the date of Easter Sunday for any year, A. D., New Style.

Let c denote the complete hundreds of years in the number denoting any year, and y the number of remaining years. Thus in the year 1883, $c = 18$ and $y = 83$, the number for the entire year, 1883, being denoted by $100\,c + y$.

In the following formulas w, as a subscript after a division, denotes that only the whole number of the quotient is to be retained, and r, as a subscript, denotes that only the remainder after the division is to be retained; thus $\left(\frac{18}{4}\right)_w = 4$; and $\left(\frac{18}{4}\right)_r = 2$.

n (the golden number *less* one)

$$= \left(\frac{\text{year}}{19}\right)_r = \left(\frac{100\,c + y}{19}\right)_r = \left(\frac{5\,c + y}{19}\right)_r = \left(\frac{5\,c + y}{20 - 1}\right)_r$$

$$= \frac{1}{19}\left[5\left(\frac{c}{19}\right)_r + \left(\frac{y}{19}\right)_r\right]_r$$

This number (n) pertains to a lunar cycle of 19 years.

$$s = \overline{c - 8} - \left(\frac{c}{4}\right)_w - \left(\frac{c + 1 - \left(\frac{c+8}{25}\right)_w}{3}\right)_w$$

Inspection of the formula for s will show that, for any year from 1700 A. D. New Style to 1899 A. D., both inclusive, the value of s is zero (0). For any year Old Style the value of s is the constant number 22.

$$q = \left(\frac{23 + s + 19\,n}{30}\right)_r \text{; also,}$$

$$= \left(\frac{23 + s - 11\,n}{30}\right)_r$$

$$h = \left(\frac{q}{29}\right)_w + \overline{29 - q}\left(\frac{q}{28}\right)_w\left(\frac{n}{11}\right)_w$$

The value of h may be shown to be zero (0) for any year from 1700 A. D. to 1899 A. D., both inclusive, during New Style, and for all years during Old Style.

$p = q - h =$ the interval in days from March 21st to the date of the Paschal full moon, or the number of days to be added to March 21st to find the date of the Paschal full moon.

If $p =$ zero (0), the Paschal full moon accordingly falls on the 21st of March.

$$L = \left(\frac{1 + 2\left(\frac{c}{4}\right)_r - y - \left(\frac{y}{4}\right)_w + \left(\frac{c}{40}\right)_w}{7} \right)_r$$

L denotes the number (in alphabetical order) of the Dominical or Sunday letter. Thus, the number corresponding to the Dominical letter A is 1, to B is 2, to C is 3, to D is 4, to E is 5, to F is 6, and to G is 7 or 0 (zero).

The term $\left(\frac{c}{40}\right)_w$ gives a correction to the Gregorian value when the year exceeds 4000 A. D.; for any year less than 4000 the value of this corrective term is obviously zero (0).

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_r = \left(\frac{3 + 6p + L}{7}\right)_r$$

t denotes the number of days which elapse after the date of the Paschal full moon to the date of Easter Sunday.

$$\begin{aligned} \text{Easter Sunday} &= \text{March } (21 + 1 + p + \overline{t - 1}) \\ &= \text{March } (21 + p + t) \\ &= \text{April } \ (p + t - 10) \end{aligned}$$

To find the date of Easter Sunday for any year, A. D., Old Style.

$$n = \left(\frac{\text{year}}{19}\right)_r = \left(\frac{100\,c + y}{19}\right)_r$$

The formula for n is the same as in New Style.

$$q = p = \left(\frac{23 + 22 + 19\,n}{30}\right)_r = \left(\frac{15 + 19\,n}{30}\right)_r = \left(\frac{15 - 11\,n}{30}\right)_r$$

$$L = \left(\frac{3 + c - y - \left(\frac{y}{4}\right)_w}{7} \right)_r$$

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_r = \left(\frac{3 + 6p + L}{7}\right)_r$$

$$\begin{aligned}\text{Easter Sunday} &= \text{March } (21 + 1 + p + \overline{t-1}) \\ &= \text{March } (21 + p + t) \\ &= \text{April } \ (p + t - 10)\end{aligned}$$

EXAMPLE 1.—*Required the day of the month on which Easter Sunday falls in the year* 1883 *A. D., New Style.*

$$n = \left(\frac{5\,c + y}{19}\right)_{\mathrm{r}} = 5\left(\frac{c}{19}\right)_{\mathrm{r}} + \left(\frac{y}{19}\right)_{\mathrm{r}}$$

$$\left(\frac{18}{19}\right)_{\mathrm{r}} = 18 \text{ or } -1\,;\ \ 5\left(\frac{18}{19}\right)_{\mathrm{r}} = 90 \text{ or } -5$$

$$\left(\frac{83}{19}\right)_{\mathrm{r}} = \left(\frac{4 \times 19 + 7}{19}\right) = 7$$

$$n = -5 + 7 = 2$$

$$20\,n = 40$$

$$19\,n = 20\,n - n = 38$$

$$s = \overline{18 - 8} - \left(\frac{18}{4}\right)_{\mathrm{w}} - \left(\frac{18 + 1 - \left(\frac{18 + 8}{25}\right)_{\mathrm{w}}}{3}\right)_{\mathrm{w}}$$

$$= 10 - 4 - \left(\frac{19 - 1}{3}\right)_{\mathrm{w}} = 10 - 4 - 6 = 0$$

$$q = \left(\frac{23 + s + 19\,n}{30}\right)_{\mathrm{r}} = \left(\frac{23 + 0 + 38}{30}\right)_{\mathrm{r}} = 1$$

$$h = \left(\frac{1}{29}\right)_{\mathrm{w}} + \overline{29 - 1}\left(\frac{1}{28}\right)_{\mathrm{w}}\left(\frac{2}{11}\right)_{\mathrm{w}} = 0 + 28 \times 0 \times 0 = 0 + 0 = 0$$

$$p = q - h = 1 - 0 = 1$$

$$L = \left(\frac{1 + 2\left(\frac{18}{4}\right)_{\mathrm{r}} - 83 - \left(\frac{83}{4}\right)_{\mathrm{w}} + \left(\frac{18}{40}\right)_{\mathrm{w}}}{7}\right)_{\mathrm{r}}$$

$$= \left(\frac{1 + \overline{2 \times 2} - 83 - 20 + 0}{7}\right)_{\mathrm{r}} = \left(\frac{1 + 4 - 6 - 6}{7}\right)_{\mathrm{r}} = 0$$

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_{\mathrm{r}} = \left(\frac{3 - 1 + 0}{7}\right)_{\mathrm{r}} = 2$$

$$\begin{aligned}\text{Easter Sunday} &= \text{March } (21 + 1 + p + \overline{t-1}) \\ &= \text{March } (22 + 1 + 2) \\ &= \text{March } 25\end{aligned}$$

EXAMPLE 2.—*Required the date of Easter Sunday for the year* 1884 *A. D., New Style.*

$$5\left(\frac{18}{19}\right)_{\rm r} = -5$$

$$\left(\frac{84}{19}\right)_{\rm r} = 8$$

$$n = 8 - 5 = 3$$

$$20\,n = 60$$

$$20 - n = 19\,n = 57$$

$$s = 0$$

$$q = \left(\frac{23 + 0 + 57}{30}\right)_{\rm r} = 20$$

$$h = \left(\frac{20}{29}\right)_{\rm w} + \overline{29 - 20}\left(\frac{20}{28}\right)_{\rm w}\left(\frac{3}{11}\right)_{\rm w}$$

$$= 0 + 9 \times 0 \times 0 = 0 + 0 = 0$$

$$p = q - h = 20 - 0 = 20$$

$$L = \left(\frac{1 + 2\left(\frac{18}{4}\right)_{\rm r} - 84 - \left(\frac{84}{4}\right)_{\rm w} + \left(\frac{18}{40}\right)_{\rm w}}{7}\right)_{\rm r}$$

$$= \left(\frac{1 + 4 - 0 - 0 + 0}{7}\right)_{\rm r} = 5$$

$$t - 1 = \left(\frac{3 - 20 + 5}{7}\right)_{\rm r} = \left(\frac{8 - 6}{7}\right)_{\rm r} = 2$$

Easter Sunday = March (21 + 1 + 20 + 2)
= March 44
= April (44 − 31)
= April 13

EXAMPLE 3.—*Required date of Easter Sunday for the year* 3966 *A. D., New Style.*

$$\begin{array}{rcr|lcl} & & 39 & 66 & & \\ 2 \times 19 & = & 38 & 57 & = & 3 \times 19 \\ & & 1 & 9 & & \end{array}$$

$$n = \left(\frac{5\,c + y}{19}\right)_{\rm r} = \left(\frac{5 \times 1 + 9}{19}\right)_{\rm r} = 14$$

$$20\,n = 280$$

$$19\,n = 20\,n - n = 266$$

$$s = \overline{39-8} - \left(\frac{39}{4}\right)_{w} - \left(\frac{\overline{39+1} - \left(\frac{39+8}{25}\right)_{w}}{3}\right)_{w}$$

$$= 31 - 9 - \left(\frac{40-1}{3}\right)_{w} = 31 - 9 - 13 = 9$$

$$q = \left(\frac{23 + s + 19\,n}{30}\right)_{r} = \left(\frac{23 + 9 + 266}{30}\right)_{r} = 28$$

$$= h\left(\frac{28}{29}\right)_{w} + \overline{29-28}\left(\frac{28}{28}\right)_{w}\left(\frac{14}{11}\right)_{w}$$

$$= 0 + 1 \times 1 \times 1 = 0 + 1 = 1$$

$$p = q - h = 28 - 1 = 27$$

$$L = \left(\frac{1 + 2\left(\frac{39}{4}\right)_{r} - 66 - \left(\frac{66}{4}\right)_{w} + \left(\frac{39}{40}\right)_{w}}{7}\right)_{r}$$

$$= \left(\frac{1 + \overline{2 \times 3} - 3 - 2 + 0}{7}\right)_{r} = 2$$

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_{r} = \left(\frac{3 - 27 + 2}{7}\right)_{r} = 6$$

Easter Sunday $=$ March $(21 + 1 + 27 + 6)$
$=$ March 55
$=$ April $(55 - 31 =)$ 24

EXAMPLE 4.—*Required the date of the Paschal full moon (March $\overline{21 + p}$), and the date of Easter Sunday (March $\overline{21 + p + t}$ or March $\overline{21 + 1 + p + (t - 1)}$ for the year* 2152 *A. D., New Style.*

$$\left(\frac{2152}{19}\right)_{r} = 5 = n$$

$$\begin{array}{rr} 19\,n = & 95 \\ s = & 1 \\ 23 = & 23 \\ \hline 19\,n + s + 23 = & 119 \end{array}$$

$$q = \left(\frac{19\,n + s + 23}{30}\right)_{r} = \left(\frac{119}{30}\right)_{r} = 29$$

$$h = 1 + \overline{0 \times 0 \times 0} = 1$$

$$p = q - h = 28$$

Paschal full moon = March $(21 + 28 =)$ 49

= April $(49 \dot{-} 31 =)$ 18

$$L = \left(\frac{1 + 2\left(\frac{21}{4}\right)_r - 52 - \left(\frac{52}{4}\right)_w + \left(\frac{21}{40}\right)_w}{7} \right)_r$$

$$= \left(\frac{1 + \overline{2 \times 1} - 52 - 13 + 0}{7} \right)_r$$

$$= \left(\frac{1 + 2 + 0 - 3 - 6}{7} \right)_r = \left(\frac{3 - 2}{7} \right)_r = 1$$

$$t - 1 = \left(\frac{3 - 28 + 1}{7} \right)_r = 4$$

Easter Sunday = March $(21 + 1 + 28 + 4 =)$ 54

= April $(54 - 31 =)$ 23

The Julian or Old Style Calendar was established by the Council of Nice A. D. 325; the first year of the Gregorian or reformed calendar was A. D. 1582, and the first year in which the reformed calendar was adopted in England was A. D. 1752.

In Russia, and in other countries where the religion of the Greek Church now obtains, the New Style of reckoning has *not* been adopted, but the Old Style is still in force.

In Alaska, Old Style was employed until after the cession of that country by Russia to the United States in the year 1869.

EXAMPLE 5.—*Find the date of Easter Sunday for the year* 1582 *A. D., Old Style.*

$$\begin{array}{rl} 15 \,| & 82 \\ 5 + 15 \,| & = 75 \\ \hline 20 - 1) & 157 \qquad (7 \\ & 140 - 7 \\ \hline & 17 + 7 = 24 = 19 + 5 \\ & n = 5 \end{array}$$

$$19\,n = (20 - 1)\,n = 100 - 5 = 95 = 3 \times 30 + 5$$

$$q = \left(\frac{19\,n + 15}{30} \right)_r = \left(\frac{5 + 15}{30} \right)_r = 20$$

$$L = \left(\frac{3 + 15 - 82 - 20}{7} \right)_r$$

$$= \left(\frac{18 - 5 - 6}{7} \right)_r = \left(\frac{18 - 11}{7} \right)_r = 0$$

$$t - 1 = \left(\frac{3 - 20 + 0}{7}\right)_r = 4$$

$$\begin{aligned}\text{Easter Sunday} &= \text{March } (22 + 20 + 4 =)\, 46 \\ &= \text{April } (46 - 31 =)\, 15\end{aligned}$$

232D MEETING. MARCH 24, 1883.

Vice-President WELLING in the Chair.

Forty-three members and visitors present.

The first communication was by Mr. J. R. EASTMAN on

THE FLORIDA EXPEDITION FOR OBSERVATION OF THE TRANSIT OF VENUS.

[Abstract.]

The observing station of the Florida expedition was upon Way Key, the largest of the group of islands known as Cedar Keys.

The principal instruments employed were a portable transit, a five-inch equatorial telescope, and a photoheliograph. The first two require no description. The photoheliograph consisted of an objective of five inches aperture and about forty feet focus, a heliostat for throwing the sun's rays on the objective, and a plate holder at the focus of the objective. The accessory apparatus consisted of a measuring rod, permanently mounted, for accurately measuring the distance from the objective to the photograph plate; a movable slide with a slit of adjustable width, for exposing the plates; and a circuit connecting with a chronograph, so arranged that when the exposing slide was moved to expose the plate, and when the center of the slit was opposite the center of the plate-holder, the circuit was broken and the record made on the chronograph. A black disk was painted on one side of the slide, and so placed that when the slide was at rest at one end of its course and the image of the sun was adjusted concentric with this disk, it would fall on the center of the plate-holder when the slide was moved. The adjustments having been completed the exposing of the plates was a simple matter. The image of the sun was thrown by the heliostat upon the black disk and centered, the sensitive plate was fixed in

the plate-holder, the operator moved the exposing slide, and the time of exposure was recorded on the chronograph.

For observing contacts I used an eye piece, magnifying 216 diameters, attached to a Herschel solar prism, and a sliding shade-glass with a density varying uniformly from end to end. The time of my signals was taken by assistant astronomer Lieut. J. A. Norris, U. S. N., from a chronometer; while, with an observing key, I also made a record on the chronograph as a check.

About 40 seconds before the computed time of first contact a narrow stratus cloud passed on to the southeastern edge of the sun and shut out all the light. The cloud remained about 3 minutes, and when it passed off, the notch in the sun's limb was plainly marked. Two photographs were taken to test the apparatus and the plates, and then the time before *second* contact was devoted to an examination of the limbs of Venus and the sun. Both were perfectly steady. In observations of the sun for the last twenty years I never saw it better. At about 13 minutes after first contact the outline of the entire disk of Venus could be seen, and seemed perfectly circular. About 2 minutes later a faint, thin rim of yellowish light appeared around the limb yet outside the sun. This rim was at first broadest near the sun's limb, but soon the width of the light became uniform throughout. The light was wholly exterior to the limb of Venus; that is, the black limb of Venus on the sun and the dark limb outside formed a perfectly circular disk, with the rim of light or halo, outside the portion off the sun. As the time of second contact approached, Lieutenant Norris again took up his station at the chronometer. As the limbs neared geometrical contact, the cusps of sunlight began to close around Venus more rapidly; and the perfect definition of the limbs and the steady, deliberate, but uniformly increasing motion of the cusps, convinced me instantly that the phenomena attending the contact would be far more simple than I had ever imagined. I had only to look steadily to see the cusps steadily but rapidly extend themselves into the thinnest visible thread of light around the following limb of Venus and remain there without a tremor or pulsation. At the moment the cusps joined I gave the signal and also made the record on the chronograph. Still keeping my eye at the telescope, I saw nothing to note save the gradually increasing line of light between the limbs of the two bodies. The disk of Venus on the sun was black.

A re-examination was then made of all the photographic apparatus, and about 10 minutes after the second contact the principal photographic work was commenced; and this was continued with slight interruption until about 10 minutes before third contact; 150 dry plates and 30 wet ones being exposed. One of the interruptions was for the purpose of making measurements of the diameter of Venus, which was done with a double-image micrometer attached to the 5-inch telescope.

On going to the telescope to observe the last contacts, I found the limbs of Venus and the sun as steady as in the morning, and though there was now some haze over the sun it did no harm. The third contact was observed with great accuracy, nothing occurring to obstruct or complicate the very simple and definite phenomena, which were in the reverse order of those seen at second contact. The rim of light appeared around Venus as soon as the limb was visible beyond the sun, and was seen for nearly 10 minutes. The complete outline of Venus was visible for 2 minutes longer. No phenomena worthy of note were seen between third and fourth contacts. The lapping of the limb of Venus over that of the sun gradually but steadily decreased until the final separation, which was observed with great accuracy for such a phenomenon. Soon after the last contact the entire apparatus was again carefully examined and the necessary observations made to determine the errors of the chronometers.

In the observations of interior contacts there was no trace of any tremor or fluctuation of the light in the cusps as they closed around the limb of Venus; and it is almost needless to say that there was no trace of a shadow or a black drop or ligament between the limbs at second and third contacts. The probable error for the second and third contacts was estimated at $0''.3$; for fourth contact, $0''.5$.

Observers of transits of Venus and Mercury have written so much in regard to the obstacles encountered from the apparition of the shadow, or black drop, between the limbs of the two bodies at *second* and *third* contacts, and so full has been the testimony in favor of the existence and the almost necessary occurrence of this phenomenon, that at the transit of Mercury, in 1878, many observers claimed, as evidence of their skill, that they *did* see it; while others, less fortunate, apologized for *not* seeing it. Observers of the black drop were so generally confined to those with imperfect

apparatus or to those unaccustomed to observation of the sun's limb or disk that the true nature of the obstacle was pretty well understood before it was carefully investigated. It is now quite well settled that the "black drop" is due to bad eyes, imperfect apparatus, or the inexperience of the observer. With good eyes and proper apparatus a good observer never should see the black drop. When it is seen there is something wrong; it is a spurious phenomenon.

One of the negatives was exhibited to the Society.

In reply to a question by Mr. E. J. Farquhar, Mr. Eastman said the halo about Venus was believed to be due to the atmosphere of the planet.

The next communication was by Mr. Cleveland Abbe on

DETERMINING THE TEMPERATURE OF THE AIR.

He stated that the question now to be considered is not where to place a thermometer so as to obtain the temperature most proper for the use of the meteorologist, but is rather the purely physical question of how to determine the temperature of the air at any given location. He described the methods and defects of the former and present meteorological methods of exposure, viz: (1) Thermometers hung in the open air. (2) Those placed in shady locations. (3) The Glaisher screen. (4) The Stevenson screen and the double louvre screens in general. (5) The double metallic cylindrical shelters of Jelinek and Wild. (6) The silver thimble screen of Regnault. (7) The whirling thermometer of Saussure, Arago, Bravais, and the French observers (exhibiting Babinet's arrangement as made by Casella.) (8) Joule's method, depending on a balance in the temperature and density of two columns of the air.

He then gave a description of the method devised by him in 1865 and used for a short time at Poulkova; this consisted in constructing a very perfect louvre screen, within which were established black bulb and bright or silvered bulb thermometers having very diverse coefficients of radiation and conduction. These thermometers were in air, not *in vacuo*, as this latter arrangement was proper only for the determination of the direct solar radiation, as in the Arago-Davy method, whereas in the present case the temperature of the air and the radiation from terrestrial objects were the special objects of study.

The air temperature (t_a) was found from the indications of the bright and black bulbs (t_s and t_b) by the empirical formula

$$t_a = t_s + C\ (t_b - t_s)$$

where C is a small coefficient, to be determined experimentally, and is nearly constant. This arrangement of bright and black bulbs can be used by meteorologists and physicists without a screen, and even in the sunlight, if the theory of the action of the bright and black bulbs is perfectly understood. A similar formula will give the temperature (T) of a single radiating body whose effect is equal to the total effect that is shown by the black bulb:

$$T = t_b + C'\ (t_b - t_s.)$$

He then stated that the theoretical basis of this method has quite recently been further elucidated by Professor Ferrel, who has shown that the approximate nature of the relation between the above constant C, the radiating, absorbing, and conducting powers of the thermometers, and the velocity of the wind is given by the following equation:

$$C = \frac{1 + \dfrac{Br_b}{B' + B''v}}{\dfrac{r_b}{r_s} - 1}$$

where r_b and r_s are the radiating (and absorbing) powers of the blackened and silvered bulbs, respectively, v is the velocity of the wind or currents flowing past the bulbs, and B B′ B″ are constant coefficients depending on the size, conductivity, and specific heat of the substance of the bulbs.

In reply to a question of Mr. GILBERT, he stated that the difference between the bright and black bulbs had rarely exceeded a few tenths of a degree in the delicate shelter made of oiled paper, as used by him at Poulkova, the maximum occurring February 22, 1866, at 10 a. m., when, the louvre box being in the full sunshine, the bright bulb was at 14°.9 Cent. and the black bulb at 14°.3, showing that the latter had been slightly warmed by the warm sides of the box.

In reply to a question of Mr. HARKNESS, the author explained, that although it was conducive to accuracy that these thermometers should be placed within a shelter, yet this was not necessary; if we take advantage of the more accurate method of determining

the co-efficient constant C, as given by Prof. Ferrel's theory, the two thermometers placed anywhere within doors or without would still give data for determining temperatures of the location; it should be borne in mind that the temperature thus obtained belongs specifically to the air in contact with the themometers and is not an average value for any extensive portion of the atmosphere. As it is an advantage to conduct observations under uniform conditions, it is recommended that a pair of bright and black bulb thermometers be attached to the whirling table, whereby the effect of a current of air may be on the one hand determined and on the other hand kept as uniform as possible.

Mr. HARKNESS said that the object practically sought by meteorologists was to learn the average temperature of a considerable body of air, but their efforts were thwarted by the irregularity and inconstancy of the distribution of temperature. So long as the air in contact with the thermometer is not precisely representative of the air of the vicinage it was useless to refine methods of observation, unless by that refinement errors of a constant nature were eliminated. For the determination of mean monthly or annual temperatures he considered the reading of the nearest half degree as sufficient, and regarded the reading of the tenths of a degree as a useless refinement.

The advantage of reading to tenths was further discussed by Messrs. ABBE, DOOLITTLE, and KUMMELL. Mr. KUMMELL pointed out that where a difference of temperature is observed as an indication of the moisture of the air, the tenths are worthy of record.

The following communication by Prof. CHARLES E. MUNROE, of Annapolis, Md., was then read by the Secretary:

DETERMINATION OF THE SPECIFIC GRAVITY OF SOLIDS BY THE COMMON HYDROMETER.

Having occasion some time since to devise methods for the examination of coal on board ship, I was obliged, as my first consideration, to work with such materials and apparatus as are usually found in ships' stores, and then to arrange the methods so that they could be used under the restricted conditions which prevail. The unsteadiness of the ship makes balance methods for the determination of specific gravities difficult, even when a suitable balance is at

hand, while hydrometers may be steadied so that the instrument may be read with a reasonable degree of precision, as is shown in its constant use in the determination of the degree of saturation of the water in the steam-boiler, and in other instances.

To use the hydrometer for the determination of the specific gravities of solids I take advantage of the fact that, when a body floats in a liquid in which it is wholly immersed, the specific gravities of the liquid and the solid are the same, and we have simply to determine the value for one of them.

The process is carried out by taking a dense solution, dropping in it the solid to be determined, (which must be light enough to float on the surface,) and then diluting slowly with water until the solid floats immersed, stirring the mixture constantly. The solid is now removed and the hydrometer inserted and read. For the determination of the specific gravities of the bituminous coals and lignites a thick solution of cane sugar was used, while for the heavier anthracite concentrated sulphuric acid, diluted with dilute sulphuric acid, was employed. The increase in temperature in the latter case causes no appreciable error if the reading is quickly taken. The following results were obtained by the method described, the specific gravity of each specimen having first been determined by Jolly's balance:

	By Jolly's balance.	By mixture.
Anthracite	1.5640	1,560
Bituminous coal	1,3008	1,310
Bituminous coal	1,3000	1,300
Gas coal	1,2790	1,285
Cannel coal (ligniform)	1,1550	1,155
Cannel coal	1,1292	1,120
Lignite	1,0909	1,090

Mr. Dutton remarked that the same principle had recently been successfully applied to the separation of the component minerals of crystalline rocks. A sample is powdered and then placed in a very heavy liquid (a solution of mercuric iodide and potassium iodide), the density of which is gradually diminished, until the particles of the heaviest mineral sink to the bottom. A repetition of the process eliminates each mineral in turn.

233D MEETING. APRIL 7, 1883.

Mr. WM. H. DALL in the Chair.

Thirty-six members and visitors present.

The Chair announced that Messrs. EDWARD SANDFORD BURGESS and SUMNER HOMER BODFISH had been elected members.

The General Committee reported to the Society that "a Mathematical Section had been organized by the election of Mr. ASAPH HALL as Chairman and Mr. HENRY FARQUHAR as Secretary. All members of the Society who are interested in mathematics are invited to attend and take part in its meetings, announcements of which will be sent to those who notify the Secretary of a desire for them."

The first communication was by Prof. W. C. KERR on

THE GEOLOGY OF HATTERAS AND THE NEIGHBORING COAST.

[Abstract.]

The notable projection of Hatteras, beyond the general line of trend of the Atlantic coast, has, of course, a geological origin. The study of the changes now taking place, and of the phenomena which have left their recent traces on the surface, readily furnish the data for the solution of the problem. Nearly one-half of this eastern inter-sound region of North Carolina is water surface, and the land surface lies for the most part below ten feet (much of it below five.)

A large part of this low-lying surface is covered with beds of peat, which thicken towards the centre on the divides or swells between the bays and sounds, rising, in some cases, to ten and fifteen feet, and in the Dismal Swamp on the northern border of the State to twenty-two feet. These beds of peat are in process of forming by the decay of plants growing on the surface, chiefly cypress and juniper. Many tiers of the undecayed logs of these timbers are piled upon one another through the whole thickness of the deposit, which is soft and yielding, so that a fence-rail may be thrust down beyond its length. Vast tracts of such peat swamps (and of marsh and savanna on which only water grasses and small shrubs and scrub pines grow and decay) are found throughout this coast region. Here we have the first stage in the formation of a coal bed. Another notable fact is that many of the rivers which empty into the sounds

increase in depth of channel at a distance from their mouths; while the sounds are 12 to 15 and 20 to 22 feet deep, the rivers are often 30 and 40 feet and upwards. This can only be accounted for by supposing a subsidence of the region to be in progress, the sounds and open bays being silted up by the deposits brought down by the floods of the Roanoake and other large rivers, while no particle of sediment can reach the sheltered depths of the narrow windings of the upper reaches of these minor streams. This theory of subsidence is abundantly confirmed by the disappearance under water of large tracts of swamp bordering the rivers, as the Chowan, within the observation of men now living, and by the existence of rooted stumps of cypress and juniper in the bottom of the bays and sounds, even to the depth of 15 and 20 feet, and also by the vertical and crumbling shores of the sounds, undermined and eroded by the advancing waves.

The Atlantic ocean is walled off from this region by a narrow fringe of sand islands, or dunes, blown shoreward by the wind and thrown up into reefs and hillocks like snow-drifts 50, 80, and even more than 100 feet high. The movement of these sand waves being inland, the sounds are silting up next the sea, and are in many places converted into marshes 3 to 5 miles wide. The reef is increasing in continuity and breadth, most of the inlets above Hatteras that were open 300 years ago being closed and obliterated. An inspection of the form of the curves of the submarine contours off Hatteras and adjoining coasts will show that the action of the tides and ocean currents, the Gulf stream and Arctic current meeting at this point, accumulate upon Hatteras the river silt which reaches the sea by way of the Chesapeake as well as that of the rivers which discharge their burdens through the inlets about this point and southwards. Which amounts to this—that Hatteras may be described as a sort of *delta*, whose materials are derived from the drainage of more than 100,000 square miles of the Atlantic slope.

A subsidence of about 20 feet would bring the sea again over the entire Sound region and carry the shore 75 miles inland, bringing Hatteras to coincide with Cape Lookout. A sand reef, like that north of Hatteras, marks the line of the ancient shore, when these conditions obtained. A depression of fifty feet would move the shore 100 miles west of Hatteras and carry the point of meeting of the conflicting ocean currents and waves to Cape Fear. A subsidence of 500 feet, as in the glacial period, would carry

Hatteras more than 200 miles west of its present position. This horizon is marked by an immense sand reef, still retaining its wind and wave marks, and rising to a height of more than 500 feet above tide, the reef itself being at least 100 feet deep and many miles in length. The sea must have remained at this level for a very long period.

But Hatteras is not a modern phenomenon. It is at least as old as the cretaceous; the quaternary as well as the tertiary of this coast region of North Carolina are laid down upon an eroded surface of cretaceous rock, while the artesian borings, at Charleston, reach this formation at 700 feet, and at the mouth of the Chesapeake they do not seem to have touched it at 1,000 feet.

Mr. WARD remarked that, in traversing the Jericho canal of the Dismal Swamp in a row boat, he had observed an outward flow at both ends of the canal, showing that, by continuous water passage, a divide was crossed between Lake Drummond and the James river.

He criticised the doctrine taught in text-books and popular writings that the preservation of leaves in a fossil state is due ordinarily to river action and delta formation. More favorable conditions are to be found in swamps.

Other remarks were made by Messrs. DUTTON and HOUGH.

The second communication was by Mr. H. F. WALLING on

TOPOGRAPHICAL INDICATIONS OF A FAULT NEAR HARPER'S FERRY.

[Abstract.]

A description was given of a break in the continuity of the Blue Ridge, where its disconnected portions, extending side by side for a few miles, are cut by the Potomac river, near Harper's Ferry, the gorges so formed presenting a striking feature of the scenery.

The two ridges, here about 12,000 feet apart, stretch for hundreds of miles in nearly parallel directions, one to the south and the other to the north; the latter being known in Pennsylvania as the South Mountain. The strike of the rocks is parallel to the ridges, about N. 30° E., and the prevailing dip is eastward, averaging not more than 30°. The ridges are composed of hard sand-rock; the adjacent region, of lime-stone and other rocks more easily disintegrated or dissolved.

Supposing the sand-rock of the Blue Ridge and South Mountains to have been originally a continuous formation, it will be readily

seen that a vertical fault in easterly dipping strata, having its direction somewhat nearer the meridian than the present strike and its downthrow on the west side of the fault, would produce a lateral discontinuity like that here observed, the upthrown part of any stratum cropping out on the east of the downthrown part at a distance depending upon the amount of the vertical displacement.

All this would depend upon whether the sand rocks were originally continuous in the two ridges—a question which was left for the geologists to decide. The writer, however, took occasion to suggest that great longitudinal faults might be formed near coast lines when the gradual overloading of the balanced crust by depositions of sediment produced a strain too great to be relieved by flexure. A rupture would then occur, the strata going down on the overloaded side of the fault and up on the other until equilibrium of pressure upon the yielding magma below was restored by lateral displacement of the magma. The fault so formed would present a diminished resistance to dislocation, and if the action which originated it should continue, it would be likely to increase in dimensions both in length and in the amount of vertical displacement. This action might even continue after the emergence of the region above the surface of the water, provided a more rapid denudation of the landward than of the seaward side of the fault took place, in which case a continued disturbance of equilibrium would be accompanied by vertical yielding, increasing the amount of dislocation, and by subterranean movements of the supporting magma, whereby a restoration of material would be effected from overloaded to denuded areas.

Moreover, the hypothesis of a constant restoration of disturbed equilibrium makes it easier to understand why the folding of strata should grow steeper, even to a *folding under*, as the axis of a mountain chain is approached. A diagram exhibiting the so-called "fan-like structure of the Alps," enlarged from a figure by Rogers, (see Rogers' Report on the Geology of Pennsylvania, Vol. II, p. 902,) was shown in illustration. The gradual subterranean movements inward under a mountain chain, as the upper portions were removed and the remainder elevated, would carry the strata along on a support of diminishing width until they were folded upward and backward.

The gradual increase towards the east in the amount of corrugation and steepness of dips, together with the supposed reversed folding by which the rocks of the eastern part of the Appalachian region seem to

dip under older rocks, still further east appear, therefore, to favor the notion that the paleozoic rocks of the Appalachian region and the eastern part of the Mississippi basin were derived from the erosion of highlands formerly existing east of the Appalachian chain, now, perhaps, submerged in the Atlantic ocean. The downthrow of a fault, if formed in the manner supposed in the region under consideration, would accordingly be on its western side, as suggested above.

The third communication was by Mr. S. F. EMMONS on

ORE DEPOSITION BY REPLACEMENT.

[Abstract.]

After a few introductory remarks upon the relatively unsatisfactory condition of that branch of geology which treats of ore deposits, considering the early date at which it was taken up, the speaker briefly reviews the existing theories and classifications, and shows that they are mainly based on the idea that each ore deposit is the filling of some pre-existing cavity or opening in the rock in which it is now found; that so-called fissure veins, for instance, were once actually open cracks, and that irregular deposits in limestone have been made by the filling up of open caves, such as so frequently occur in these rocks. The result of his studies of the so-called "carbonate deposits" of Leadville, Colorado, has been to show that they are *not* the filling up of pre-existing cavities; the caves there have been formed since the ore was deposited, as is proved by their crossing indiscriminately ore bodies and limestone. They belong to a class of deposits for which he proposes the name *metamorphic deposits,* or those which have been formed by a metasomatic interchange between the vein and original rock material. In Leadville the principal deposits are an actual replacement of the limestone itself at or near the contact of this stratum with an overlying sheet of porphyry. This replacement action has in places proceeded so far that the entire stratum of ore-bearing limestone or dolomite, originally 150 to 200 feet thick, has been changed into vein material, which consists of silica and metallic minerals. This vein material was brought in solution by percolating waters, which had taken it up during their circulation through the adjoining and generally overlying eruptive rocks. A more detailed description of the phenomena of these deposits will be found in his paper en-

titled "Abstract of a Report on the Geology of Leadville," in the Second Annual Report of the Director of the United States Geological Survey.

While the speaker's studies have thus far been mainly confined to limestone deposits, he has reason to believe that essentially the same process has produced a large proportion of ore deposits in crystalline and eruptive rocks, and that to the class of metamorphic deposits belong most of the so-called fissure veins of the Rocky Mountain region. That is, that they are not the filling in of pre-existent open fissures by vein materials foreign to the adjoining rocks, but simply a metamorphic change of these rocks themselves along channels of easy access to percolating waters; and according to the character of the material held in solution by these waters, these rocks have been more or less changed into quartz and metallic minerals, to a greater or less width, as the case may be. Numerous instances of such veins will be found in the forthcoming Census Report upon the Statistics and Technology of the Precious Metals, by Mr. G. F. Becker and the speaker.

234TH MEETING. APRIL 21, 1883.

Vice-President BILLINGS in the Chair.

Forty members present.

The Chair announced that Messrs. WASHINGTON CARRUTHERS KERR and SAMUEL FRANKLIN EMMONS had been elected members.

Mr. W. H. DALL addressed the Society on

GLACIATION IN ALASKA,

illustrating his remarks by maps of the territory and of the glacial areas of the St. Elias Alps and Kachekmak Bay, Cook's Inlet, the latter being from surveys made by him under the direction of the U. S. Coast Survey.

He called attention in the first place to the wide differences in the character of the masses of ice resulting from the consolidation of snow by gravity (which would usually be classed as glaciers), as observed by him during nine years' exploration in Alaska.

These might be classed under several heads: as plateau-ice, filling

large areas of depression and without motion as a whole, but when sufficiently accumulated overflowing the edges of its basin in various directions; as valley-ice, filling wide valleys of gentle incline both as to their axes and their lateral slopes, producing masses of ice moving in a definite direction but without lateral and sometimes even without terminal moraines; as ice-cascades, formed in sharp narrow ravines of very steep inclination, usually without well-defined surface moraines; as typical glaciers, showing névé and lateral and terminal moraines; and lastly, as effete or fossil glaciers, whose sources have become exhausted, whose motion has therefore ceased, and whose lower portions have become smothered by the accumulation of non-conducting débris. The very existence of one of these last has remained unknown for half a century, though the plateau underwhich it is buried has been described and mapped by explorers.

Another form under which ice appears in Alaska is that of solid motionless layers, sometimes of great thickness, interstratified with sand, clay, etc. A deposit probably of this character is described by Nordenskiöld, on the Asiatic coast, near Bering Strait. In Alaska this formation, in which ice plays the part of a stratified rock, extends from Kotzebue sound, where the greatest known thickness of the ice-layer, about three hundred feet, has been noted, around the Arctic coast, probably to the eastern boundary. In Kotzebue Sound the ice is surmounted by about forty feet of clay containing the remains of fossil horses, buffaloes (*Bos latifrons*, etc.), mountain sheep, and other mammals. Farther north the ice is covered with a much thinner coat of mineral matter or soil, usually not exceeding two or three feet in thickness, and rarely rises more than twelve or fifteen feet above high water mark on the sea coast. Its continuity is broken between Kotzebue Sound and Icy Cape by rocky hills composed chiefly of carboniferous limestones, which bear no glaciers and do not seem to have been glaciated. The absence of bowlders and erratics over all this area has been noted by Franklin, Beechey, and all others who have explored it. The remarkable extent and character of the formation was unknown previous to the speaker's investigations, though the ice cliffs of Kotzebue Sound had attracted attention from the time of their first discovery.

Mr. Dall desired especially to emphasize the distinction between these strata of pure ice and the "frozen soil" so often alluded to by arctic explorers. The absence of frozen soil in the alluvium

of the Yukon Valley, far north of Kotzebue Sound, was noted, as well as the fact that this valley has, for some unexplained reason, a mean temperature considerably above the normal, so that its forests extend well beyond the Arctic circle.

The distribution of glaciers, properly so-called, in Alaska, as far as our present knowledge goes, is confined to the region of the Alaskan range and the ranges parallel with it south of the Yukon Valley, but particularly to the coast mountains bordering on the Gulf of Alaska and the Alexander Archipelago, of which the Saint Elias Alps form the most conspicuous uplift.

The distribution of stratified ice is all north of the Yukon Valley, which divides the two regions. Hence, for the glacial epoch, it may be presumed that the one is the equivalent of the other, and the fact that Arctic Alaska is marked by stratified ice, rather than glaciers such as those of Greenland, must be due to local geological and climatic peculiarities existing at the time. On the Asiatic coast, especially at Holy Cross Bay, in nearly the same latitude and with not very different topographic conditions, glaciers are abundant at the present time.

On the mainland, facing the Alexander Archipelago, especially toward Lynn Canal, Icy Strait and the Stikine region, local glaciers are abundant, and traces of others, now dissolved, may be found on the lowlands of most of the islands. That these were always local, though doubtless very extensive, and that they were the progeny of the topography instead of being its parent, is obvious to anyone who has seen the coasts of Maine or Norway, which have been submitted to general glaciation, and will compare their rounded, worn, and *moutonnée* aspect with that of the sharp cliffs, beetling crags, narrow valleys, and scanty lowlands of the Alaskan islands.

The speaker concluded, from his observations, that the extent of the Alaskan glaciers is greatly diminished from its former state, and is probably still diminishing; that the southern portion of the Territory is probably nearly or quite stationary, while the northern part is undergoing elevation; and that, from the nature of the case, the area of stratified ice cannot be expected to increase or diminish materially without changes in geological or climatic conditions too great to be anticipated.

Mr. Alvord remarked that on Point Barrow frozen ground had been penetrated to a depth of thirteen feet.

In reply to a question by Mr. ANTISELL, Mr. DALL said that little was known of the humidity of the interior of Alaska; 23 inches of precipitation, nearly all in snow, had been observed in a single year at one point and 12 inches at another.

Mr. F. B. HOUGH then read a paper on

THE CULTIVATION OF THE EUCALYPTUS ON THE ROMAN CAMPAGNA,

which was discussed by Messrs. E. B. ELLIOTT and H. FARQUHAR. It is published in the American Journal of Forestry for June, 1883.

235TH MEETING. MAY 5, 1883.

Vice-President BILLINGS in the Chair.

Twenty-seven members and visitors present.

The Chair announced the election to membership of Messrs. WILLIAM THOMAS SAMPSON, JOHN OSCAR SKINNER, and THOMAS CROWDER CHAMBERLIN.

The first communication was by Mr. H. A. HAZEN on

HYGROMETRIC OBSERVATIONS.

[Abstract.]

After describing the various devices by which the moisture of the air has been measured, and especially the novel and valuable apparatus of Crova, the speaker illustrated the difficulty of the subject by contrasting synchronous determinations made at four points within a radius of two miles, and then described some experiments tending to show the inaccuracy of the wet and dry bulb hygrometer, as ordinarily observed. The value of the wet bulb reading is enhanced by blowing on the bulb with a bellows, or otherwise subjecting it to a brisk current of air.

Mr. HARKNESS remarked first, that Mr. Hazen's experiments appeared to prove the insufficiency of Regnault's formula, for they showed the difference between the indications of the wet bulb and dry bulb to be a function not only of the humidity, but of the velocity of wind; second, that height of station above the ground

was a condition to which too little attention had been given; and third, that there seemed a possibility of obtaining a slightly erroneous vapor tension with Crova's apparatus.

Mr. E. J. Farquhar then read a paper on

DREAMS IN THEIR RELATION WITH PSYCHOLOGY.

[Abstract.]

Several theories of dreams were considered and none found entirely sufficient; not because a new and complete one was to be proposed, but because all seemed a little too partial and limiting in their scope. After touching on the relation of dreams to sleep and to waking, as intermediate between them, discrediting many recorded experiments on the ground of their being vitiated by a special purpose latent in the mind, and pointing out that the usual supposition of our being often waked by the intensity of a dream appears to put cause for effect, since it must be the fact of waking that effects the dream, perhaps by slow degrees—the character of mental operations in dreams was discussed. Dissent was expressed from the opinion that the dreaming state is devoid of such originating power as belongs to the waking; this position was maintained by showing first, the extreme vividness and lastingness of impression often pertaining to dreams, apart from any features of horror; then the coherence, far from being unknown among them, yet of a peculiar kind; and, finally, the true significance occasionally appearing in them, generally by figurative shape, amounting sometimes to a real enlightenment of the mind. Regarding the faculties or aspects of mind most apt to display themselves in dreams, it was held that all were liable to the exercise in turn, though some of the higher ones, especially the moral sense and judgment, less than others; since these expressed a rarer and more distinctive force evolved and laid up by and for our relations with actual life, while other powers whose exercise is less of an expenditure from the most important vitalities of mind were freer at the time—the principles of conservation and struggle for existence being thought to apply among the mental elements. Thus, to a certain degree, the mind may be seen more clearly in its true character by means of dreams than awake, though in very partial views at a time. Unconscious mental action was reviewed in this connection, and it was held that not only the lower processes, called reflex, but many of the highest functions

largely partake of this attribute. A great number of other points in regard to dreams were merely named as illustrating the fertility of the subject.

236TH MEETING. MAY 19, 1883.

Vice-President HILGARD in the Chair.

Forty members and visitors present.

It was announced from the General Committee that the following rules had been adopted:

I. If the author of any paper read before a section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee, to be appointed as the section may determine. The report of this committee shall be forwarded to the Publication Committee by the secretary of the section, together with any action of the section taken thereon.

II. Any paper read before a section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

Mr. ROBERT FLETCHER made a communication entitled

RECENT EXPERIMENTS ON SERPENT VENOM.

It is published in the *American Journal of the Medical Sciences* for July, 1883.

Mr. H. FARQUHAR then made a communication on

FURTHER EXPERIMENTS IN BINARY ARITHMETIC,

showing that the relation between the vertical and horizontal dimensions of the characters used in the binary notation is a factor in determining its economic value. He presented, also, the results of a series of comparative tests showing that the binary notation enables some persons, after brief practice, to perform addition more rapidly than with denary notation, while with others it requires a longer time. The latter class includes practiced computers, generally, and the former those less accustomed to the use of figures.

Mr. DOOLITTLE remarked that the most instructive results would be obtained by experimenting with young persons; and the subject was further discussed by Messrs. W. B. TAYLOR, E. B. ELLIOTT, and C. A. SCHOTT.

237TH MEETING. JUNE 2, 1883.

Vice-President HILGARD, and afterward Mr. HARKNESS, in the Chair.

Twenty-two members present.

It was announced that the next meeting would be held October 13th.

Mr. W. LEE made a communication, with illustrations, entitled

SKETCHES FROM MEDALLIC MEDICAL HISTORY.

[Abstract.]

The paper was prefaced by remarks on the value of coin and medal collecting as a profitable means of instruction, and by a recognition of the danger to which collectors are exposed of developing a mania for collecting odd and curious things which cease to be instructive. An extended interest in numismatics commenced to show itself in this country in 1858, at which time there were probably not as many as one hundred coin collectors in the United States. The interest has grown rapidly, however, until now there must be on the books of the United States Mint the names of at least one thousand collectors who receive yearly the issue of the mint, with a special proof polish. In New York alone, during the year 1882, there were thirty-nine collections sold at public auction, the amount realized being $68,441.36. The largest of these was the Bushnell collection, which realized $13,900.47. Several of our large cities have numismatic societies, some of which are designated as numismatic and archæological societies; and a number of periodicals devoted simply to the interest of numismatics obtain a satisfactory circulation.

The modes of striking off coins and medals were given somewhat in detail, and attention was then called to the important part which medals struck in honor of medical men and to commemorate im-

portant events bearing directly upon the history of medicine have played throughout the history of the world. The illustrations of the paper included a hundred and fifty examples of the medals themselves, in regular sequence, from the days of Roman and Greek medicine down almost to the date of the paper itself, an interesting commemoration of events and individuals marking epochs in the history of medicine. These medals were taken up *seriatim*, references were made to the lives of individuals and the scientific work done by them, and descriptions were given of the occasions which called for the striking of medals.

The paper closed with an expression of hope that the Society might be stimulated at the sight of so many handsome and permanent memorials of the men and times of the past, to attempt to preserve the features of its first president, Joseph Henry, in a similar enduring form.

The bibliography of the subject was discussed at some length, and the following works were referred to:

MEAD, Richardi.—Dissertatio de Nummis quibusdam a Smyrnaeis in medicorum honorem percussis. Naples, 1752.

RUDOLPHI, C. A.—Index numismatum in virorum de rebus medicis vel physicis meritorum memoriam percussorum. Berlin, 1st edition 1823, 2d edition 1825, 12mo., XII, 131 pp, 3d edition 1828, 4th edition 1829. (This work (2d edition) comprises the description of 523 medals struck in honor of 350 scientific and medical men.)

RENAULDIN, Leop. Jos.—Études historiques et critiques sur les Médicins Numismatistes, contenant leur biographie et l'analyse de leurs écrits. Paris, 1851, 8°, XVI, 574 pp. (This work contains the names of 61 physicians).

CHÉREAU (A).—Les mereaux et les getons de l'ancienne faculté de médecine de Paris. L'Union Médicale. Paris, 1873, 3 Series, XV, pp. 309, 321.

PFEIFFER, (L) und RULAND (C).—Pestilentia in Nummis. Geschichte der grossen Volkskrankheiten in numismatischen Documenten. Ein beitrag zur Geschichte der Medicin und der Cultur. Tubingen, 1882, 8° X, 189 pp. Mit zwei Tafeln Abbildungen in lichtdruck.

WROTH, Warwick.—Asklepios and the Coins of Pergamon. From the Numismatic Chronicle and Journal of the Numismatic Society. London, 1882, Part I, Third Series, No. 5, pages 1 to 51, plates 3.

MOEHSEN, J. C. G.—The exact title of this author's work is not known to the writer of the paper; it was written in German,

and embodies a description of a collection of medals in Berlin struck in honor of physicians, giving 200 medals struck after the 15th century.

GROTEFEND, C. L.—Die Stempel der Römischen Augenärzte. Hannover, 1867.

Mr. T. N. GILL then made a communication on

ANALOGUES IN ZOO-GEOGRAPHY.

238TH MEETING. OCTOBER 13, 1883.

The Society, in accordance with the notice of adjournment at the June meeting, resumed its sessions.

The President in the Chair.

Forty-four members and visitors present.

It was announced that during the vacation the Society had lost by death Surgeon General C. H. CRANE, one of its Vice-Presidents; Admiral B. F. SANDS, one of its founders; and Dr. JOSIAH CURTIS.

It was further announced from the General Committee that Mr. GARRICK MALLERY had been appointed Vice-President to fill the vacancy occasioned by the death of Mr. CRANE, and that Mr. C. V. RILEY had been added to the General Committee to complete its number.

Mr. WILLIAM B. TAYLOR read a paper entitled

NOTE ON THE RINGS OF SATURN.

[Abstract.]

After an historic sketch of the varying and apparently incongruous observations by astronomers on the markings and aspects of the Saturnian rings, down to those of Schiaparelli of the Milan Observatory, (published in June last,) Mr. TAYLOR remarked that since the mathematical discussion by Prof. J. Clerk Maxwell, in 1857,* both the rigid and the fluid ring theories have been abandoned; and the discrete or meteoric constitution of the rings is now accepted by all physical astronomers as conclusively established.

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* *On the Stability of the Motion of Saturn's Rings.* 4to. 71 pp. and 1 plate. Cambridge, Eng., 1859.

Reference was then made to the startling announcement by Otto Struve, in 1851, that a careful comparison of the earlier with the later measurements showed that during the two hundred years of observation the rings had been widening, and the inner edge steadily approaching the body of the planet.* Considering the necessarily vast antiquity of the Saturnian system, such a change during the brief interval of human existence seems *à priori* almost infinitely improbable. The hypothesis of some that a meteoric ring has been drawn in by Saturn's attraction, within comparatively recent ages, seems entirely negatived by the circular symmetry of the system. It is not surprising, therefore, that Struve's inference has been received with an almost universal incredulity by the astronomical world. Robert Main, of the Greenwich Observatory, from a discussion of his own measurements taken in the winter of 1852–'3, and in 1854, disputed the accuracy of Struve's measures; and concluded that "no change has taken place in the system since the time of Huyghens."† And Prof. F. Kaiser, in a paper on "The Hypothesis of Otto Struve respecting the gradual increase of Saturn's Ring," etc., arrives at the same conclusion, and believes "there exists no reason whatever for supposing that the compound ring of Saturn is gradually increasing in breadth." ‡

There seems to be little doubt of some unintentional exaggeration in Struve's tabulated results, which range from 4″.6 : 6″.5 for the ratio of ring breadth to space between ring and ball, in the time of Huyghens, 1657, to 7″.4 : 3″.7 for the ratio of breadth to space, by his own observation in 1851. Nevertheless it is a noteworthy fact that all the early drawings of Saturn made in the seventeenth century (many of which are figured by Huyghens in his *Systema Saturnium*, 1659) plainly exhibit the width of the ring as sensibly less than the dark space within; while all modern observers would agree that the bright ring is now wider than the dark space, in about the ratio of 3 : 2; or were we to take the average of the esti-

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**Recueil des Mèmoires prèsentès* [etc.] *par les Astronomes de Poulkova.* 4to. St. Petersburg, 1853. Vol. I, pp. 349–385. "Sur les Dimensions des Anneaux de Saturne." (Memoir read before Acad. Sci.) A brief abstract of the memoir is given in the *Monthly Notices, R. A. S.*, November 12, 1852. Vol. XIII, pp. 22–24.

† *Monthly Notices, R. A. S.*, December 14, 1855. Vol. XVI, pp. 30-36.

‡ *Mem. Acad. Sci., Amsterdam*, 1858. A translation of the memoir is given in the *Monthly Notices, R. A. S.*, January 11, 1856. Vol. XVI, pp. 66–72.

mates of the last century, it would probably not vary far from $5''.25 : 5''.75$; while the general average for the present century would probably be about $6''.5 : 4''.5$. There seems, therefore, to be a real difference, not accounted for by inferiority of earlier instruments and estimates, nor by the existing uncertainties of modern measurements. The question will probably be definitely settled in less than a century. Meanwhile there is a need of some explanation of the apparently systematic and progressive divergence first pointed out by Struve; and we naturally ask, What indications are afforded by theory?

The elder Herschel, in 1789, (at the Saturnian equinox, when the edge of the ring was presented to view,) from supposed observation of protuberances moving on the line, believed that he had detected a rotation, whose period he estimated at 10h. 32m. 15s., for the outer edge of the ring.* The correctness of this interpretation was controverted by Schroeter, from observations at Lilienthal, on the next passage of Saturn's equatorial node in 1803; as it was afterward questioned by Prof. G. P. Bond, of Harvard Observatory, from observations in 1848.† It is scarcely doubtful that Herschel's period was derived from an entire misconception of the nature of the ring—which he firmly held to be solid—and that it possesses no scientific value whatever. A. Secchi, from certain recurrent irregularities of phase observed at Rome in 1854, 1855, and 1856, inferred a rotation period of 14h. 23m. This is doubtless a nearer approximation (for the outer edge of the ring) than Herschel's estimate. It is not probable, however, that the period of any portion of the ring will be determined by observation.

Accepting the meteoric theory of the rings as now established, we may by Kepler's law compute with confidence the period of rotation of any part of the ring; and we thus find—

From the period of the inner Satellite (*Mimas*)	22h.	37½m.—
The period of outer edge of ring	14h.	30 m.
" dividing stripe	11h.	20 m.
" inner edge of bright ring	7h.	12 m.
" inner edge of dusky ring	5h.	45 m.
Mean period of ring (supposed solid) about	10h.	50 m.

The period of the planet Saturn is . . . 10h. 14m.

* *Phil. Trans. Roy. Soc.* 1790: Vol. LXXX, p. 479; and 1792: Vol. LXXXII, p. 6.

† Gould's *Astronomical Journal.* 1850. Vol. I, pp. 20, 21.

Thus regarding each constituent element of the ring as having its own independent rotation, (a condition absolutely essential to the stability of the system,) we may consider that from the complicated and variable perturbations by the exterior satellites, no one particle can revolve in a circular orbit, and hence that in a space so crowded there must be a considerable amount of interference. The collisions at intersecting orbits may result in heat or in disintegration; but in any event they must tend to a degradation of motion, and hence to a slightly shortened mean radius-vector and a shortened period.

Theoretically then such an effect as that indicated by Struve would seem inevitable, whether as a matter of fact it has been sufficient in a couple of centuries to be detected or not. And this involves a modified conception as to the earlier condition of the Saturnian rings. To suppose a fine web of nebulous matter continuously spun out from Saturn's equator, with an unchanging balance of centrifugal and centripetal forces during the long ages while the planet was slowly contracting to one-half its radius, is certainly no easy task or plausible theory. If, however, we are now beholding but a stage of transitional development of the ring, we shall have to imagine its primitive radius considerably larger, and its width as probably very much narrower—so narrow indeed as to have a planetary or satellitic status, revolving in a single definite period—possibly that of *Mimas* the nearest satellite. Such a ring would present a condition of comparatively great stability; and it may have been that only the secular recurrence of rare and remarkable conjunctions commenced upon it the work of disturbance and disintegration.

When Galileo, the first to see the strange appendages to Saturn, (though without being able to distinguish the *ansæ* as parts of a ring,) observed, in 1612, that they had entirely disappeared, he wrote in some dismay, "Has Saturn possibly devoured his own children?" * So may perhaps the future astronomer, seeing but an airy trace of the historic ring, repeat the saying, Saturn has indeed devoured his offspring; not indeed completely, for a part will probably still remain; nor with violent catastrophe, for the scattered fragments falling by their eccentricity will be absorbed as gently as are the meteors daily falling on our earth.

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* Third letter to Marc Velser, December 1, 1612. *Opere di Galileo.* 4to. 4 vols. Padua, 1744: Vol. II, p. 123.

A subsidiary point deserving of notice is the certainty that the inner portions of the bright ring (and still more those of the dusky ring) are revolving in periods three or four hours shorter than that of Saturn himself. When Professor Hall made his brilliant discovery of the satellites of Mars, and announced that the inner satellite (*Phobos*) was found to have the short period of 7h. 38m. (or less than one-third of that of Mars) the fact was at once proclaimed by some as incompatible with the "nebular hypothesis." Everybody knows that the rotation periods of the sun and planets do not conform to the third law of Kepler. Our own moon has an actual velocity in its orbit more than double that of our terrestrial equator. And had the moon a little less than one-third its present distance, (that is, were its radius-vector less than 70,000 miles,) its *angular* velocity would exceed that of the earth, or its period would be less than 24 hours. Or, stated in another way, our earth, if expanded to the orbit of the moon, (under the most favorable disposition of form and of homogenous density,) would occupy considerably more than a year in completing its rotation. The supposed nebular difficulty is therefore just as pertinent to our own satellite as to those of Saturn or of Mars. The obvious solution is, that all the planets (without exception) have lost a very large amount of rotatory energy; and this may be largely or chiefly ascribed to the retarding effects of internal friction resulting from solar tides. And, given time enough, the rotation of every planet should be finally reduced to the lunar condition of a precise accord of its diurnal and annual periods. On any hypothesis whatever, it is certain that the rotations of the planets are very much slower (notwithstanding too the acceleration due to contraction) than they originally were. This fact certainly offers no objection to the nebular hypothesis.

Mr. Dutton questioned the validity of Ennis' hypothesis, that the rotation of a nebular mass could be initiated by purely internal movements.

Other remarks were made by Mr. Frisby.

Mr. S. M. Burnett then made a communication on

THE CHARACTER OF THE FOCAL LINES IN ASTIGMATISM,

showing that the two lines which limit the focal interval of Sturm have been erroneously assumed to be straight. There is only one

special case of the triaxial ellipsoid in which they are straight. In all other cases they are curved.

The full text of this paper may be found in the *Archives of Ophthalmology*, Vol. XII, Nos. 3 and 4.

Mr. H. A. HAZEN followed with a communication on

THERMOMETER EXPOSURE.

[Abstract.]

Without entering upon the question, Where in any locality shall the air temperature be observed, it is proposed to discuss the even more important question, What shall be the environment of a thermometer that it may give the true temperature. The practice has been very various: in England the Stevenson shelter is regarded as a standard: this is a double-louvred frame, wholly of wood, 18 x 10 x 18 inches, and placed about 4 feet above grass. In Russia we find a large wooden outside shelter of single louvres open to the north, inside of which is placed a metallic screen, the whole being exposed 12 or 13 feet above grass. In any exposure we should seek, first, to allow the freest possible access of the outer air, and second, to screen the thermometer from direct sun heat, from precipitation, and from radiation, whether (*a*) from surrounding objects by day or (*b*) to the sky at night.

It is important that we adopt some ready means of accurately determining the air temperature which may answer as a standard of comparison. This we have in the swung thermometer, which, by its free motion through a large body of air shaded from direct sunlight in the daytime, is calculated to give good results.

Experiments have been tried with a so-called "Pattern" shelter constructed of wood, of single louvres, inclined 30° to the horizontal, thus giving a good air circulation. The size is 4 x 3 x 3 feet, and it is erected at a height of 13 feet above a tin roof. In order to determine the least admissible size for a shelter, thermometers were placed in the Pattern 5 inches apart and running in an east and west direction, and these were observed morning and afternoon. It has been found that with a hot sun and still air the heat from the louvres rapidly diminishes with distance and becomes insensible at 15 inches. Comparisons have also been made for several weeks between the Russian and Pattern shelters; and the means of 100 sets of continuous observations on a still day, and again on a windy day, are shown n the following table:

	Dry thermometer.		Wet thermometer.		Relative humidity; per cent.	
	Russian.	Pattern.	R.	P.	R..	P
Still air	74°.8	73°.5	64°.0	62°.7	52.4	51.1
Light south wind	77 .2	77 .1	62 .0	61 .0	36.7	34.1

These results show directly the advantage of a good circulation of air, and that after shielding from the sun and radiation to the sky with a shelter at least 3 feet long, we may neglect other considerations.

Experiments are still in progress to determine the proper height above sod or roof, the proper exposure for a north window, and so forth.

Mr. ANTISELL, referring to the general theme rather than to the special subject of the paper, took occasion to note that the practice of conducting meteorologic observations on the tops of high houses, while it may well subserve the special purposes of the Signal Service, renders their work of materially less value to the medical profession. There is so much change, especially of the moisture element, in the first few feet from the ground upward that no observations can be depended upon as reporting the conditions of the phenomena of disease unless they are made in the layer actually occupied by man.

Mr. TAYLOR asked whether there might not be an error arising from the set given to the glass of the bulb by the pressure of the mercury of a whirled thermometer.

Mr. HAZEN replied that he had tested the effect of pressure applied to the bulb with the finger, and found that the set produced was of very brief duration. He had also tested the thermic effect of the friction on the atmosphere incurred by rapid whirling, and found it inappreciable with a velocity of about fourteen miles an hour. On whirling a black bulb thermometer, he observed a change of several tenths of a degree, which appeared clearly referable to the greater coefficient of friction of the surface roughened by lampblack.

Mr. GRAHAM BELL remarked that if we eliminate radiation and learn the absolute temperature of the air at the point of observation, our knowledge is still limited to that point only, whereas for meteorologic purposes it is important to ascertain the average temperature of a body of air. He suggested the possibility of utilizing for this purpose a measurement of the velocity of sound, which

velocity is dependent on atmospheric temperature and independent of barometric pressure.

Mr. DUTTON thought that the extreme delicacy of this observation would involve an uncertainty greater than the one which now inheres in the determination.

239TH MEETING. OCTOBER 27, 1883.

The President in the Chair.

Forty-seven members and guests present.

The Chair announced the death of two members since the last meeting—LEONARD DUNNELL GALE and ELISHA FOOTE.

Announcement was also made of the election to membership of CHARLES DOOLITTLE WALCOTT.

Mr. T. N. GILL made a communication on

ICHTHYOLOGICAL RESULTS OF THE VOYAGE OF THE ALBATROSS.

Mr. ALEXANDER GRAHAM BELL made the following communication on

FALLACIES CONCERNING THE DEAF, AND THE INFLUENCE OF SUCH FALLACIES IN PREVENTING THE AMELIORATION OF THEIR CONDITION.

It is difficult to form an adequate conception of the prevalence of deafness in the community. There is hardly a man in the country who has not in his circle of friends and acquaintances at least one deaf person with whom he finds it difficult to converse excepting by means of a hearing-tube or trumpet. Now is it not an extraordinary fact that these deaf friends are nearly all adults? Where are the little children who are similarly afflicted? Have any of us seen a child with a hearing-tube or trumpet? If not, why not? The fact is that very young children who are hard of hearing, or who cannot hear at all, do not naturally speak, and this fact has given origin to the term "deaf-mute," by which it is customary to designate a person who is deaf from childhood.

"But are there no deaf children," you may ask, "excepting those whom we term deaf-mutes?" No; none. In the tenth census

of the United States (1880) persons who became deaf under the age of sixteen years were returned as "deaf and dumb." Such facts as these give support to the fallacy that deafness, unaccompanied by any other natural defect, is confined to adult life, and is specially characteristic of advancing old age.

So constant is the association of defective speech with defective hearing in childhood that if one of your children whom you have left at home, hearing perfectly and talking perfectly, should, from some accident, lose his hearing, he would also naturally lose his speech. Why is this, and why are those who are born deaf always also dumb?

Fallacies Concerning the Dumbness of Deaf Children.

The most ingenious and fallacious arguments have been advanced in explanation. George Sibscota,* in 1670, claimed that the nerves of the tongue and larynx were connected with the nerves of the ear, "and from this Communion of the vessels proceeds the sympathy between the Ear, the Tongue and Larynx, and the very affection of those parts are easily communicated one with the other. Hence it is that the pulling of the Membrane of the Ear causeth a dry Cough in the party; and that is the reason most deaf men * * * are Dumb, or else speak with great difficulty; that is, are not capable of framing true words or of articulate pronunciation by reason, of the want of that convenient influx of the animal spirits; and for this cause also, it is that those who are thick of Hearing have a kind of hoarce speech."

The value of Sibscota's reasoning may be judged of by the further information he gives us concerning the uses of the Eustachian tube. "By this it is," he says, "that Smoakers, puffing up their Bheeks, having taken in the fume of Tobacco, send it out at their Ears. Therefore the opinion of *Alcmaeon* is not ridiculous, who held that she-Goats did breathe thorough their Ears," &c., &c.

It is easy for us to laugh at the fallacies of the past, but are we ourselves any less liable to error on that account? The majority of people at the present day believe that those who are born deaf are also dumb *because of defective vocal organs.* Now let us examine

* I have been informed that Sibscota's work, "The Deaf and Dumb Man's Discourse," from which the above extracts are taken, is in reality a translation of another work by Anthony Densing, published in 1656.

this proposition. It is a more ridiculous and absurd fallacy than that of Sibscota and more easily disposed of.

The hypothesis that congenitally deaf children do not naturally speak because their vocal organs are defective involves the assumption that were their vocal organs perfect such children *would* naturally speak. But why should they speak a language they have never heard? Do we speak any language that we have not heard? Are our vocal organs defective because we do not talk Chinese? It is a fallacy. The deaf have as perfect vocal organs as our own, and do not naturally speak because they do not hear. I have myself examined the vocal organs of more than 400 deaf-mutes without discovering any other peculiarities than those to be found among hearing and speaking children. The deaf children of Italy and Germany are almost universally taught to speak, and why should we not teach ours? Wherever determined efforts have been made in this country success has followed and articulation schools have been established.

Fallacy Concerning the Intelligence of Deaf Children.

The use of the word "mute" engenders another fallacy concerning the mental condition of deaf children. There are two classes of persons who do not naturally speak—those who are dumb on account of defective hearing and those who are dumb on account of defective minds. All idiots are dumb.

Deaf children are gathered into institutions and schools that have been established for their benefit away from the general observation of the public, and even in adult life they hold themselves aloof from hearing people; while idiots and feeble-minded persons are not so generally withdrawn from their families. Hence the greater number of "mutes" who are accessible to public observation are dumb on account of defective minds, and not of defective hearing. No wonder, therefore, that the two classes are often confounded together. It is the hard task of every principal of an institution for the deaf and dumb to turn idiots and feeble-minded children away from his school—children who hear perfectly, but cannot speak. Although it is evidently fallacious to argue that, because all deaf infants are dumb, and all idiots are dumb; therefore all deaf infants are idiots: still this kind of reasoning is unconsciously indulged in by a large proportion of our population; and the majority of those who for the first time visit an institution

for the deaf and dumb express unfeigned astonishment at the brightness and intelligence displayed by the pupils.

Why Hearing Children who become Deaf also become Dumb.

I have stated above that children who are born deaf do not naturally speak because they cannot hear. For the same reason children who lose their hearing after having learned to speak naturally tend to lose their speech. They acquired speech through the ear by imitating the utterances of their friends and relatives, and when they become deaf they gradually forget the true pronunciation of the words they know, and have naturally no means of learning the pronunciation of new words; hence their speech tends to become more and more defective until they finally cease to use spoken words at all.

Adults who become deaf do not usually have defective speech, for in their case the habit of speaking has been so fully formed that the mere practice of the vocal organs in talking to friends prevents loss of distinctness. We can learn, however, from the case of Alexander Selkirk how important is constant practice of the vocal organs. This man, after about one year's solitary residence upon an island, was found to have nearly forgotten his mother tongue; and we find that deaf adults who shrink from society and use their vocal organs only on rare occasions acquire peculiarities of utterance that are characteristic of persons in their condition, although the general intelligibility of their speech is not affected.

Fallacies Regarding the Nature of Speech.

The fallacies I have already alluded to respecting the difference between those who become deaf in childhood and those who become deaf in adult life have their origin in a fallacy concerning the nature of speech itself. To most people, who do not reflect upon the subject, it appears that speech is acquired by a natural process similar to that by which we acquire our teeth. At a certain age the teeth make their appearance, and at another age we begin to talk. To unreflecting minds it appears that we *grow into speech;* that speech is a natural product of the vocal organs, produced without instruction and education ; and this leads directly to the fallacy that where speech is wanting or imperfect the vocal organs are defective.

I have already stated that this cause has been assigned in expla-

nation of the dumbness of children who are deaf. The idea gives rise also to the popular notion that stammering and other defects of speech are diseases to be "cured," and the attempt has been made to do so, even by heroic treatment. It is not so very long ago that slices have been cut from the tongue of a stammerer, in the vain hope of "curing" what was, after all, but a bad habit of speech. I have myself known of cases where the uvula has been excised to correct the same defect. The dumbness of the deaf and the defective speech of the hearing are some of the penalties we pay for acquiring speech ignorantly, by mere imitation. If parents realized that stammering and other defects of speech were caused by ignorance of the actions of the vocal organs, and not necessarily through any defect of the mouth, they would have their children taught the use of the vocal organs by articulation teachers, instead of patronizing the widely-advertised specialty physicians, who pretend by secret means to "cure" what is not a disease. Speech is naturally acquired by imitation, and through the same agency defects of speech are propagated. A child copies the defective utterance of his father. A school-fellow mocks a stammering companion, and becomes himself similarly affected. In the one case the fallacy that the supposed disease is hereditary prevents attempts at instruction and correction, and in the other the idea that the affliction is the judgment of God in the way of punishment discourages the afflicted person and renders him utterly hopeless of any escape excepting by a miracle.

A practical illustration of the fact that defective speech is propagated by imitation is shown in my own case. When I was a boy my father was a teacher of elocution, and had living with him at one time one or two pupils who stammered. While under the care of my father, these boys spoke clearly and well, without any apparent defect, but, owing to his being called away for a protracted period of time, his pupils relapsed, and the boys commenced to stammer as badly as at first. Upon my father's return he found a house full of stammerers. *His own sons were stammering too!* I can well remember the process of instruction through which I went before the defect was corrected in my own case.

Ignorance the Real Difficulty in the Way of Teaching Deaf Children to Speak.

Speech is the mechanical result of certain adjustments of the

vocal organs, and if we can teach deaf children the correct adjustments of the perfect organs they possess, they will speak. The difficulty lies with us. We learn to speak by imitating the sounds we hear, in utter ignorance of the action of the organs that accompanies the sounds. I find myself addressing an audience composed of scientific men, including many of the most eminent persons in the country, and I wonder how many there are in this room who could give an intelligible account of the movements of their vocal organs in uttering the simplest sentence? We must study the mechanism of speech, and when we know what are the correct adjustments of the organs concerned, ingenuity and skill will find the means of teaching *perfect* articulation to the deaf.

The Old Fallacy—" Without Speech, no Reason."

I have already stated that children who are born deaf are also always dumb. How, then, can they think? It is difficult for us to realize the possibility of a train of thought being carried on without words; but what words can a deaf child know, who has never heard the sounds of speech?

When we think, we think in words, though we may not actually utter sounds. Let us eliminate from our consciousness the train of words, and what remains? I do not venture to answer the question; but it is this, and this alone, that belongs to the thoughts of a deaf child.

It is hardly to be wondered at, therefore, that the fallacy should have arisen in the past that there could be no thought without speech; and this fallacy prevented for hundreds of years any attempt at the education of the deaf. Before the end of the last century deaf-mutes were classed among the idiots and insane; they had no civil rights, could hold no property; they were irresponsible beings. Even those interested in the religious welfare of the world consigned their souls to the wrong place, for "faith comes by hearing," and how could a deaf child be saved? I say that for hundreds of years the old fallacy, that "without speech there could be no reason," hindered and prevented any attempt at the amelioration of the condition of the deaf. But, strange to say, it was this very fallacy that first led to their education. It was attempted, by a miracle to teach them to speak.

In Bede's History of the Anglo-Saxon church we read "How Bishopp John cured a dumme man with blessing him."

"And when one weeke of Lent was past, the next sounday he willed the poore man to come unto him; when he was come, he bydd him put out his tounge and show it unto him, and taking him by the chinne, made the signe of the holy crosse upon his tounge, and when he had so signed and blessed it, he commaunded him to plucke it in again, and speake saying, speake, me one word, say *gea, gea*, which in the english tounge is a worde of affirmation and consent in such signification as yea, yea.* Incontinent the stringes of his tounge were loosed, and he said that which was commanded him to say. The bishopp added certain letters by name, and bid him say A; he said A; say B, he said B, and when he had said and recited after the bishopp the whole cross rewe he put upon him sillables and hole wordes to be pronounced. Unto which when he answered in all pointes orderly, he commaunded him to speake long sentences, and so he did; and ceased not all that day and night following, so longe as he could hold up his head from sleepe (as they make report that were present) to speake and declare his secret thoughtes and purposes, which before that day he could never utter to any man."†

Now, stripped of the miraculous, this is simply a case of articulation teaching. In the other countries of Europe the first attempts at the education of the deaf were also made by teaching them to speak, and as the early teachers were monks of the Roman Catholic Church, it is probable that these schools resulted from the attempts to perform the miracle of healing the dumb. A large proportion of the deaf and dumb who were thus brought together were successfully taught to articulate.

But now comes a marvel: It was found by the old monks that their pupils came to understand the utterances of others by watching the mouth. Such a statement appears more marvelous to those who understand the mechanism of speech than to those who are ignorant of it; and there is a general tendency to consider this accomplishment as among the fictitious embellishments of the old narratives. But the experience of modern teachers confirms the fact. John Bulwer, who is said to have been the earliest English writer upon the subject of the instruction of the deaf and dumb, published

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* It will be remembered that the original of this was in Latin, and that "the english tounge" here means what we now call the Anglo Saxon.

† American Annals of the Deaf and Dumb, vol. I, p. 33 (1848).

in the year 1648 a treatise entitled "Philocophus; or, the Deaf and Dumbe Man's Friend. Exhibiting the Philosophicall verity of that subtile Art, which may inable one with an *observant Eie*, to *Heare* what any man speaks by the moving of his lips. Upon the same Ground, with the advantage of an Historicall Exemplification, apparently proving, That a Man Borne Deafe and Dumbe may be taught to *Heare* the sound of *words* with his *Eie*, and thence learn to speak with his tongue."

Articulation Teaching in America.

In Europe at the present time deaf children are much more commonly taught to speak and understand speech than in this country.

In the majority of our schools and institutions articulation and speech-reading are taught to only a favored few, and in these schools no use is made of articulation as a means of communication. A considerable number of the deaf children in our institutions could once hear and speak, and those pupils who retain some knowledge of spoken language have their vocal organs exercised for an hour or so a day in an articulation class under a special articulation teacher, but this is not enough exercise to retain the speech. I have seen a boy who became deaf at 12 years of age, and who had previously attended one of our public schools, go into an institution for the deaf and dumb talking as readily as you or I *and come out a deaf mute.*

Few, if any, attempts are made to teach articulation to those who have not naturally spoken, except at the special request of parents who desire that the experiment shall be tried with their children.

I have seen a congenital deaf mute, who also had a sister deaf and dumb, who was taught to speak in adult life, and I found upon experiment that he could understand by ear the words and sentences that he had been taught to articulate when they were spoken in an ordinary tone of voice about a foot behind his head, yet this young man had been educated at one of our best institutions without acquiring articulation, and as a consequence he grew up a deaf mute and married a deaf mute. He informed me himself that he could hear the people talking in the workshop where he was employed, but did not understand what they said.

As a matter of personal observation I am convinced that a large proportion of the congenitally deaf are only hard of hearing, and this belief is supported by the fact that it used to be the custom in

some of our institutions to summon the pupils from the play-ground *by the ringing of a bell!* Does this not indicate that a large number of the pupils could hear the ringing of the bell, and that they told the others who could not hear at all? Such pupils could have been taught to speak at home by their friends if artificial assistance had been given to their hearing. There was no necessity for their ever becoming deaf and dumb.

It is only within the last fifteen years or thereabouts that schools have been established in the United States where all the deaf children admitted are taught articulation and speech-reading, but such schools are rapidly increasing in number. Still, it is not generally known that the experimental stage has passed, and that all deaf mutes can be taught intelligible speech. This is now done in Italy and Germany, and the international conventions of teachers of the deaf and dumb held recently at Milan and Brussels have decided in favor of articulation for the deaf.

I have stated before that the difficulties in the way of teaching articulation are external to the deaf. They lie with us and in our general ignorance of the mechanism of speech. A teacher who does not himself understand the mechanism of speech is hardly competent to produce the best results. So dense is the general ignorance upon this subject that it is probable that of the 50,000,000 of people in this country the number of persons who are familiar with all that is known concerning the mechanism of speech might be numbered on the two hands. Considering this, the success obtained in our articulation schools is gratifying and wonderful.

Upon the Art of Understanding Speech by the Eye.

It has been found in the articulation schools of this country that deaf children can acquire the art of understanding by eye the utterances of their friends and relatives, and this fact has led some teachers to suppose that speech is as clearly visible to the eye as it is to the ear, and this fallacy tends to hinder the acquisition of the art by their pupils.

When we examine the visibility of the elementary sounds of our language we find that the majority can not be clearly distinguished by the eye. How then, you may ask, can a deaf child who cannot distinguish the elements understand words which are combinations of these elements?

When the lips are closed we cannot see what is going on inside

the mouth. The elementary sounds of our language, represented by the letters P, B, and M, involve a closure of the lips. Hence the differences of adjustment that originate the differences of sound are interior and cannot be seen. But while the deaf child may not be able to say definitely whether the sound you utter is P, B, or M, he knows certainly that it must be one of these three, for no other sounds involve a closure of the lips. And so with the other elements of our language. While he may not be able to tell definitely the particular element to which you give utterance, he can generally refer it to a group of sounds that present the same appearance to the eye. In the same manner he may not be able to tell the precise word that you utter, but he can refer it to a group of words having the same appearance. For instance, the words "pat," "bat," and "mat" have the same appearance to the eye. While he cannot tell which of these words you mean when it is uttered singly, he readily distinguishes it in a sentence by the context. For instance, were you to say that you had wiped your feet upon a "mat," the word could not be "pat" and it could not be "bat."

Here we come to the key to the art of understanding speech by the eye—Context. But this involves, as a prerequisite, a competent knowledge of the English language; and we may particularly distinguish those children who have acquired the art from those who have not, by their superior attainments in this respect. We can, therefore, see why children who have become deaf after having learned to speak, naturally acquire this power to a greater extent than those who are born deaf.

There are many cases of congenitally deaf children who have acquired this art as perfectly as those who have become deaf from disease; but in every case such children have been thoroughly familiar with the English language, at least in its written form.

Fallacies Regarding Speech-reading.

The fallacy that speech is as clearly visible to the eye as it is audible to the ear hinders the acquisition of the art by causing the teacher to articulate slowly and word by word, even opening the mouth to its widest extent to make the actions of the organs more visible. When we realize that context is the key to speech-reading, theory asserts that ordinary conversational speech should be more intelligible than slow and labored articulation. This is amply proved by the experience of the most accomplished speech-readers.

I have been told by one who has acquired this art that when introduced to strangers their speech is more readily understood if they are not aware they are speaking to one who cannot hear. The moment they are told they commence to speak slowly and open their mouths to an unnatural extent, thus rendering their articulation partially unintelligible. The change brought about by the knowledge that the listener could not hear was sometimes sudden and great.

I have lately made an examination of the visibility of all the words in our language contained in a small pocket dictionary, and the result has assured me that there are glorious possibilities in the way of teaching speech-reading to the deaf, if teachers will give special attention to the subject.

One of the results of my investigation has been that the ambiguities of speech are confined to the little words, chiefly to monosyllables. The longer words are nearly all clearly intelligible. The reason is obvious, for the greater number of elements there are in a word, the less likelihood is there that another word can be found that presents exactly the same outline to the eye.

We need never be afraid, therefore, of using long words to a deaf child, if they are within his comprehension. We are apt to have the idea that short words will be simpler, and we sometimes try to compose sentences consisting as much as possible of monosyllabic words, under the impression that such words are easy for the pupil to pronounce and read from the mouth. It is more common, therefore, to present such sentences to beginners than to more advanced pupils. Now, I do not mean to say that these sentences may not be easier for a child to pronounce, but the words used are the most ambiguous to the eye. Such a simple word as "man," for instance, is homophenous with no less than thirteen other words.

A few years ago I dictated a string of words to some pupils, with the object of testing whether they judged by context or were able to distinguish words clearly by the eye. The results are instructive. Among the words dictated occurred the following: "Hit—rate—ferry—aren't—hat—four—that—reason—high—knit—donned—co." I told the pupils not to mind whether they understood what I said or not, but simply to write down what they thought the words looked like, and what do you think they wrote? Upon examining their slates I found that nearly every child had written the following sentence: "It rained very hard, and for that reason

I did not go." I told the pupils to be very careful to observe whether they could distinguish any difference between the words I uttered and the words they wrote. I therefore went over the whole string of words again, articulating them one by one very distinctly. No difference whatever was detected.

The mother of one of my pupils was present, and was greatly astonished to see her daughter writing down words so different from those I had pronounced. She said that she could not have believed that her daughter could have been so stupid; but her surprise was increased when she found that the other children had written the same sentence. I told her that there was no difference in appearance between the words I had uttered and the words they had written. She desired to test the matter herself with her own child. She asked her daughter to repeat after *her* the words I had written, but the result was the same. The last part of the sentence she repeated at least a dozen times, without shaking her daughter's confidence in the belief that the words she had uttered were precisely the same as those spoken by her mother. To one who could hear, it was a startling revelation to observe the confidence of the child in the accuracy of her replies.

"Repeat after me," said the mother, as she pronounced the words singly and with deliberate distinctness: "high;" answer, "I; "knit," ans., "did;" "donned," ans., "not;" "co," ans., "go.' "Are you sure you have pronounced the words exactly as I have said them?" Ans. "Yes; perfectly certain." "Try again." "Knit," answer, "did;" "donned," answer "not." "Are you *sure* I said that?" Ans. "Yes; absolutely sure." "Try again," and here the mother mouthed the word "donned," ans., "not." The mother was convinced, and she left the room with the remark that she felt that she had been very cruel to her child through ignorance of the fact that words that were very different to her ear looked alike to her child, and could not possibly be distinguished, excepting by context.

I have seen a teacher attempting to impart instruction to a deaf child by word of mouth. She would speak word by word, and the pupil would repeat after her. Upon one occasion the pupil gave utterance to a very different word from that which had been spoken by the teacher. The latter repeated the word a number of times, opening her mouth to the widest extent, and the boy each time repeated the incorrect expression. The teacher grew annoyed at the supposed stupidity of the pupil, and the pupil grew sulky, and was

discouraged in his attempt to read from the mouth; whereas, in reality, it was not the stupidity of the boy that was in the way of his progress, but the ignorance of the teacher, who did not know that the words that were so different to her ear were absolutely alike to his eye.

Some teachers, in their anxiety to teach speech-reading to their pupils, have the idea that they should refrain from every other mode of communication, so that their pupils may be forced to observe the movements of the mouth, and the mouth alone. For instance, it is easy to write an ambiguous word or to spell it by a manual alphabet, but some teachers refrain from doing so, under the impression that this practice leads the pupil to depend upon the hand instead of the mouth.

Again, deaf persons gather an idea of the emotion that actuates a speaker by the expression of his countenance. In fact facial expression is to the eye what the modulation of the voice is to the ear. It gives life to the inaudible utterances of the mouth; but there are some teachers who are so afraid that their pupils may come to depend upon the face instead of the mouth, that they think they should assume an impassive countenance from which nothing could be inferred.

Requisites to the Art of Speech-reading.

If we examine the visibility of speech and the causes of its intelligibility, we shall find that there are three qualifications that must be possessed by a deaf child in order that he may understand readily the utterances of his friends. Omit any one of these qualifications and good speech-reading is an impossibility:

I. The eye must be trained to recognize readily those movements of the vocal organs that are visible. Has this ever been done? Have not pupils been required to grapple with all the difficulties of speech-reading at once, and to observe not only the movements of the vocal organs, but to find out the meaning of what is said?

II. I have already explained that certain words have the same appearance to the eye, and it is necessary, if the pupil is to understand general conversation, that he shall know the words that look alike, so that a given series of movements of the vocal organs shall suggest to his mind not a single word, but a group of words, from which selection is to be made by context.

An illustration will explain what I mean. There are many

words which have the same sound to the ear, but different significations. For instance, were I to ask you to spell the word "rāne," you could not tell whether I meant "rain," "rein," or "reign." These words sound alike, but they lead to no confusion, for they are readily distinguished by context. In the same way "homophenous words," or words that have the same appearance to the eye, are readily distinguished by context.

As a general rule when a teacher finds that her pupil does not understand a given word, she supposes the non-comprehension to be due to an untrained eye, and this leads to the patient repetition of the word with widely opened mouth, to make the action of the organs more visible. This, unintentionally, enables the pupil to acquire a knowledge of homophenous words; for, when he fails to understand in the first instance, he is requested to try again. He then guesses at the meaning. He thinks of all the words that past experience has taught him looked something like the word proposed, and after a series of guesses generally succeeds in his attempt to unravel the meaning.

In this way success comes at last, not in consequence of the pupil seeing more than he saw at first, but in consequence of knowledge gained by experience of failure. He learns what words present the same appearance to the eye. Let teachers find out the words that look alike, and teach them in groups to their pupils. In this way instruction will take the place of painful experience.

III. The third requisite to good speech-reading is familiarity with the English language. Familiarity with our language, either in its written or spoken form, is absolutely essential in order that a deaf person may make use of context in his attempt to decipher our speech. It is a mental problem that the deaf child has to solve and not solely a problem of vision. The eyes of the congenitally deaf, if there is any difference at all, are rather stronger and better than the eyes of those who become deaf from disease; and yet, as a class, the congenitally deaf acquire the art of speech-reading with much more difficulty than those who could speak before they became deaf. The reason is, that, as a class, the former have not a vernacular knowledge of our language even in its written form, while the latter have. Children who become deaf in infancy from disease are at as great a disadvantage in this respect as the congenitally deaf, and for the same reason.

I shall inquire more particularly into the cause of this lack of

familiarity with the English language, and I shall show that it results from a wide-spread fallacy regarding the nature of language and the means by which our language should be taught. In the meantime I shall simply direct attention to the fact that those who are deaf from infancy do not, as a general rule, become familiar with the English language even in its written form.

It is obvious that if we talk to deaf children by word of mouth, and refrain from explaining, by writing or some other clearly visible means, the words that are ambiguous, those pupils who are already familiar with the language have very great advantages over the others. They have a fund of words from which to draw, they can guess at the ambiguous word and substitute other words within their knowledge so as finally to arrive at the correct meaning. But young children who have been deaf from infancy and who never, therefore, have known our language, are not qualified at once for this species of guess-work. They know no words excepting those we teach them, and have, therefore, no fund to draw upon in case of perplexity. If we commence the education of such children by speech-reading alone they are plunged into difficulties to which they have not the key.

To such children it becomes a matter of absolute necessity that our language should be presented to them in an unambiguous form. With such pupils, writing should be the main reliance, and speech-reading can only be satisfactorily acquired by the constant accompaniment of writing, or its equivalent—a manual alphabet. I have no hesitation in saying that the attempt to carry on the general education of young children who are deaf from infancy by means of articulation and speech-reading alone, without the habitual use of English in a more clearly visible form, would tend to retard their mental development. I do not mean to say that this is ever actually done, but I know there is a tendency among teachers of articulation to rely too much upon the general intelligibility of their speech. Let them realize that the intelligibility is almost entirely due to context, and they will rely more upon writing and less upon the mouth in their instructions to young congenitally deaf children.

After a probationary period, pupils who could speak before they became deaf become so expert in speech-reading that the regular instruction of the school-room can be carried on through its means without detriment to the pupil's progress. The exceptional cases of congenitally deaf persons who have become expert in this art

assures us that, with all who are deaf from infancy, we can certainly achieve the same results if only we can give them a sufficient knowledge of our language, at least in its written form. In the early stages of the education of the congenitally deaf it appears to me that written English should be made the vernacular of the school-room, and that all words or sentences written should also be spoken by the teacher and read by the pupils from the mouth. When the English language has become vernacular there is no reason why instruction should not also be given by word of mouth alone (as in the case of those who could speak before they became deaf) without interfering with mental development.

Before leaving this subject I would say that it is of importance to remember that speaking and understanding speech by the eye are two very different things. We can all of us speak very readily, but I fancy it would puzzle most of us to be called upon to tell what a speaker says by watching his mouth. The congenitally deaf can certainly be taught to speak intelligibly even by persons unfamiliar with the mechanism of articulation. Such pupils should therefore be taught to articulate, and their vocal organs should be continually exercised in the school-room by causing them to speak as well as to write. The congenitally deaf can be taught to articulate even *before* they are familiar with English, but I do not think they can acquire the power of understanding ordinary conversational speech by watching the mouth, at least to any great extent, until *after* they have become familiar with our language.

Gesture Language.

I have already stated that the old fallacy, "without speech there can be no reason," prevented for hundreds of years any attempt at the education of the deaf and dumb, and now I come to the memorable experiment that forever exploded the fallacy. Towards the latter end of the last century the Abbe de l'Epee, during the course of his ministration in Paris, entered a room in which two girls were sewing. He addressed some remarks to them, but received no reply. These girls were deaf and dumb. At once the kind heart of the good Abbe was touched, and he determined to devote his life to the amelioration of the condition of the deaf and dumb.

He gathered together quite a number of deaf children, who made their home with him. He spent his time in their society and devoted to their comfort all that he possessed, reducing himself even

to poverty for their sake. He soon observed that these children were communicating with one another, but not by speech. They were inventing a language of their own, unlike any of the spoken languages of the earth—a language of gestures. These children were reasoning by means of this language; they were thinking in gestures instead of in words, and the idea occurred to the Abbe de l'Epee that the old dogma that had for so many hundred years prevented the education of the deaf was a fallacy. Here was nature developing an instrument of reason with which speech had nothing to do. Why should he not study this gesture language and assist these children in their attempts to perfect a means of communication of this kind, and why should he not use this means of communication so as to lead their minds to higher and ever higher thoughts? He did so and succeeded in developing the "sign language" that is now so extensively employed in this country in the education of the deaf. The experiment at once attracted attention. Kings and Emperors visited the humble abode of the Abbe de l'Epee and were astonished by what they saw. He conversed with his pupils in the gesture language, and he taught them through its means the meaning of written French, so that they were enabled to communicate with hearing persons by writing.

The Fallacy that a Gesture Language is the only Form of Language that is Natural to the Congenitally Deaf.

The old fallacy was done away with, but a new one immediately took its place, which has been introduced into our country with the language of signs, and is now the main obstacle to the acquisition of English by the congenitally deaf. The fallacy to which I allude is that this gesture language is the only language that is natural to the congenitally deaf, and that therefore such children must acquire this language as their vernacular before learning the English language, and must be taught the meaning of the latter through its means. To my mind such a statement consists of a succession of fallacies, each one resting on the preceding. The proposition that the sign language is the only language that is natural to congenitally deaf children is like the proposition that the English language is the only language that is natural to hearing children. It is natural only in the same sense that English is natural to an American child. It is the language of the people by whom he is surrounded. A congenitally deaf child who for the first time enters an insti-

tution for the deaf and dumb finds the pupils and teachers employing a gesture language which he does not understand; but in time he comes to understand it, and learns by imitation to use it, just as an American child in Germany comes in time to understand and speak German.

Although congenitally deaf children, when they enter an institution, do not understand or use the sign language as there employed, they each know and use a gesture language of some kind, which they employ at home in communicating with their friends and relatives. Hence it is argued that if the "sign language" employed in our institutions is not the only one, a gesture language of some kind is necessarily the vernacular of the congenitally deaf child. The scope of the statement is thus widened, and the proposition we have now to consider may be thus expressed: Gesture language, in the wider sense, is the only form of language that is natural to those who are congenitally deaf.

It is a matter of great importance to the 34,000 deaf-mutes of this country, and to their friends and relatives, as well as to all persons who are interested in the amelioration of the condition of the deaf and dumb, that we examine this proposition with care and decide whether it is a fallacy or not. To my mind it is a fallacy based upon another concerning the nature of language itself, namely, that there is such a thing as a natural language. Such an idea has led to errors in the past, and will ever continue to do so. We have all read of the monarch of ancient times, who is recorded to have shut up a number of little children by themselves, and to have given orders to their attendants to hold no communication with them, so that he might observe what language they would naturally speak as they grew up. It is recorded that the first word uttered was a Greek word, from which it was argued that the Greek language was the natural language of mankind.

In the seventeenth century the ingenious Van Helmont was imbued with the idea that the Hebrew language was of divine origin, from which he argued that Hebrew was the natural language of mankind, and that the shapes of the Hebrew letters had some natural relation to the sounds they represented; that they pictured, in fact, the positions of the vocal organs in forming the sounds. The latter idea led him to employ the characters as a means of teaching articulation to a deaf-mute; but the former idea led him to teach his deaf-mute Hebrew, instead of his native tongue.

When we examine the languages of the world that are naturally acquired by hearing children, we fail to discover any natural connection between the sounds of the words and the things they represent; everything is arbitrary and conventional.

Origin and Mode of Growth of a Gesture Language.

Now, let us examine for a moment the nature of a gesture language and the manner in which it comes into existence. You are, we shall suppose, a farmer, and your little deaf boy comes running into the house in great excitement, anxious to tell you something he has observed. How does he do so?

We shall imagine a case. He commences by placing his hands above his head, bowing low, and marching about the room, after which he points out of the window.

You shake your head; you have not the remotest idea what he means.

His face assumes an anxious look, and down he goes upon his hands and knees, and scrambles over the floor, touching the carpet with his mouth from time to time, and then again points out of the window.

Still you do not comprehend.

A look of perplexity crosses his face. What can he do to make you understand? At last his face lights up, as a new thought comes into his mind, and he touches the bridge of his nose and again points out of the window.

But, alas! alas! you cannot understand.

The little fellow is perplexed and troubled. At last, in despair, he takes hold of your coat and pulls you out of the door, around the corner, and *you find your cow in the turnip patch.*

Now you begin to understand what it was he meant to say; he had tried to picture the cow, and to imitate its actions. The hands held above the head had indicated the horns; the scrambling on the floor on his hands and knees had imitated the action of a four-footed animal, and his mouth to the carpet meant the cow eating the turnips.

But how about the bridge of his nose?

You will probably observe that the cow to which he referred had some white spot or other mark upon the nose, and the gesture of the child had not indicated a cow in general, but your black cow "Bessie," with the white spot on her nose, in particular.

Having advanced thus far in the comprehension of his meaning, do you think that the child will take the trouble to go through this same pantomime the next time he wishes to tell you about your cow? No. He may commence such a pantomime, but before he gets half through you understand what he means, and he never completes it. A process of abbreviation commences, until finally a touch on the bridge of his nose alone becomes the name of your black cow "Bessie," and the simple holding of his hands above his head conveys to your mind the idea of a cow in general.

By a natural process of abbreviation the child arrives at a simple gesture or sign for every object or thing in which he is interested.

But there are many thoughts he desires to express which are abstract in their nature. How, for instance, can he indicate by any sign the color of an object? Suppose, by way of illustration, that he desired to communicate to you the idea that he had seen in the road a cow that was perfectly white?

I shall try to depict the conversation between yourself and your deaf boy as it might actually have occurred.

THE BOY. The boy points to the road, touches his teeth, and holds his hands above his head.

You gather from this a vague idea of some connection between that road, the boy's teeth, and a cow.

Here is a problem: What did he mean? It is pretty clear that he had seen a cow in the road, but what connection had his teeth with that? Perhaps the cow's teeth were peculiar. You think you had better get him to explain, so—

THE FATHER. You touch your teeth with an interrogative and puzzled look.

THE BOY. The boy responds by showing you his shirt sleeve and pointing to the road.

Can he mean that there was any connection between his shirt sleeve and the cow. To clear this point—

THE FATHER. You touch his shirt sleeve and raise your hands above your head with a look of interrogation.

THE BOY. The boy nods vigorously, raises his hands above his head, and makes his sign for "snow," followed by other signs for objects that are white.

After he has presented a sufficient number of such signs, you perceive that the one thing common to them all was their color—they

were white. And thus you gain the idea that the cow was white.

Do you suppose he goes through this process every time he desires to communicate the idea of white? No; he remembers the object which had conveyed to your mind the idea that that cow was white, and the sign for this object is ever after used as an adjective, qualifying the object the whiteness of which he desires to indicate. Of course you cannot predicate what this particular sign may be. I have seen children who have conveyed the idea by touching their teeth; others who expressed it by an undulatory downward movement of the hand, expressive of the way in which a snow-flake falls to the ground.

It will thus be understood that a deaf child first commences to express his ideas by pantomime, and that by a process of abbreviation pantomimic gestures come to be used in a conventional manner. Pantomime is no more entitled to the name of language than a picture is, although many ideas can be conveyed through its means. In proportion as it becomes more conventional and arbitrary it becomes more and more worthy of the name of language.

The Sign-Language of Our Institutions.

Now, when the deaf children who lived with the Abbe de l'Epee were first brought together, each of them used a gesture-language he had invented for himself as a means of communicating with his friends at home. Thus there were as many gesture-languages as there were children. The only element common to these languages was probably the pantomime from which they had all sprung. But now what happened? Association and the necessity of intercommunication led to the adoption of common signs. Each child presented his gestures to his fellows, and by a process of selection those signs that appeared to the majority to be most fitting survived, and were adopted by the whole; and the synonymous signs, which were not so well fitted, were either forgotten by disuse or used in a new meaning to express other ideas.

I do not wonder at the interest displayed in this growth by the Abbe de L'Epee and his contemporaries. To my mind it was the most interesting and instructive spectacle that has ever been presented to the mind of man—*the gradual evolution of an organized language from simple pantomime.*

When, in 1817, the first school for the deaf and dumb was opened in America, the sign-language as used in the school of the Abbe de

l'Epee (then under the charge of his successor, the Abbe Sicard) was imported from France, and became the medium of instruction. The teachers trained in this school naturally became the principals of other institutions established upon its model, and thus the sign-language has been diffused over the length and breadth of our land.

I heartily agree with all that experienced teachers of the deaf have urged concerning the beauty and great interest of this gesture language. It is indeed interesting to observe how pantomimic gestures have been abbreviated to simple signs expressive of concrete ideas; how these have been compounded or have changed their meaning to indicate abstract thoughts; and how the sequence of the sign-words has to a certain extent become obligatory, thus forming a sort of gesture syntax or grammar.

The original stock or stocks from which our languages are derived must have disappeared from earth ages before historic times; but in the gesture speech of the deaf we have a language whose history can be traced *ab origine,* and it has appeared to me that this fact should give it a unique and independent value. In the year 1878, in a paper read before the Anthropological Society of London, I advocated the study of the gesture language by men of science; for it seemed to me that the study of the mode in which the sign language has arisen from pantomime might throw a flood of light upon the origin and mode of growth of all languages.

You may ask why it is that, with my high appreciation of this language *as a language,* I should advocate its entire abolition in our institutions for the deaf.

I admit all that has been urged by experienced teachers concerning the ease with which a deaf child acquires this language, and its perfect adaptability for the purpose of developing his mind; but after all it is not the language of the millions of people among whom his lot in life is cast. It is to them a foreign tongue, and the more he becomes habituated to its use the more he becomes a stranger in his own country.

This is not denied by teachers of the deaf and dumb, but the argument is made, as I have stated above, that it is the only language that is natural to congenitally deaf children, or that at all events, some form of gesture language must necessarily be their vernacular, and be employed to teach our English tongue.

The Fallacy that a Gesture Language is the only form of Language in which a Congenitally Deaf Child can Think.

Now what do we mean by a language being "natural" or not? I cannot believe that in this 19th century any one really entertains the fallacy that there is a natural language *per se*. So I presume that that language is considered natural to a person in which he thinks. Under this meaning the proposition assumes this shape: The sign language taught in our institutions, or a gesture language of some kind, is the only form of language in which a congenitally deaf child can think; that is, it is the only language of which the elements can be associated directly with the ideas they express.

In this form the fallacy is easily exploded, for in the course of the last one hundred years so many experiments have been made in the education of the deaf that we now know with absolute certainty that deaf children can be taught to associate written words directly with the ideas they represent; and when they are taught to spell these words by a manual alphabet, the movements of the fingers become so natural a method of giving vent to their thoughts that even in sleep their fingers move when they dream.

Not only has written English been made the vernacular of congenitally deaf children, but the same result has been achieved with written French, German, Spanish, Dutch, and other languages.

Congenitally deaf children who have been taught articulation move their mouths in their sleep and give utterance to words when they dream.

Laura Bridgman, the blind deaf-mute, was taught by the late Dr. Howe to gather ideas through the sense of touch. English words printed in raised letters were presented to her sense of touch in connection with the objects which they represented, and she associated the impressions produced upon the ends of her fingers with the objects themselves. The English language in a *tangible* form became her vernacular.

All these facts assure us that any form of language may become natural to a deaf child by usage, so long as it is presented to the senses he possesses. There is only one way that language is naturally acquired, and that is by usage and imitation. Any form of language that can be clearly appreciated by the senses the deaf child possesses, will become his vernacular if it is used by those about him.

Why the Deaf employ a Gesture Language.

A gesture language is employed by a deaf child at home, not because it is the only language that is natural to one in his condition, but because his friends neglect to use in his presence any other form of language that can be appreciated by his senses. Speech is addressed to his ear; but his ear is dead, and the motions of the mouth cannot be fully interpreted without previous familiarity with the language. On account, therefore, of the neglect of parents and friends to present to his eye any clearly visible form of language, the deaf child is forced to invent such a means of communication, which his friends then adopt by imitation. I venture to express the opinion that no gesture language would be developed at home by a deaf child if his parents and friends habitually employed, in his presence, the English language in a clearly visible form. He would come to understand it by usage, and use it by imitation.

An old writer, George Dalgarno, in 1680, expressed the opinion, in which I fully concur, that "there might be successful addresses made to a dumb child even in its cradle, *risu cognoscere matrem*, if the mother or nurse had but as nimble a hand as usually they have a tongue."

When deaf children enter an institution they find the other pupils and the teachers using a form of gesture language which they do not understand. For the first time in their lives they find a language used by those about them that is addressed to the senses they possess. After a longer or shorter time they discard the language that they had themselves devised, and acquire, *by imitation*, the sign language of the institution.

Harmful Results of the Sign Language.

After a few months residence in the institution, the children return to their friends in the holidays using easily and fluently a language that is foreign to them, while of the English language they know no more than the average school boy does of French or German after the same period of instruction. The only language they can employ in talking to their friends is the crude gesture language of their own invention, which they had long before discarded at school; and they perpetually contrast the difficulty and slowness of comprehension of their friends with the ease with which their school fellows and teachers could understand what they mean. They have

learned by experience how sweet a thing it is to communicate freely with other minds, and they are continually hampered and annoyed by the difficulty they meet with in conversing with their own parents and friends.

Can it be wondered at, therefore, that such a child soon tires of home? He longs for the school play-ground, and the deaf companions with whom he can converse so easily. Little by little the ties of blood and relationship are weakened, and *the institution becomes his home.*

Nor are these all the harmful effects that are directly traceable to the habitual use in school, as a means of communication, of a language foreign to the mass of the people. Disastrous results are traceable inwards in the operation of his mind, and outwards in his relation to the external world in adult life. He has learned to *think* in the gesture-language, and his most perfected English expressions are only translations of his sign speech.

As a general rule, when his education is completed, his knowledge of the English language is like the knowledge of French or German possessed by the average hearing child on leaving school. He cannot read an ordinary book intelligently without frequent recourse to a dictionary. He can understand a good deal of what he sees in the newspapers, especially if it concerns what interests him personally, and he can generally manage to make people understand what he wishes by writing, but he writes in broken English, as a foreigner would speak.

Let us consider for a moment the condition of a person whose vernacular is different from that of the people by whom he is surrounded. Place one of our American school boys just graduated from school in the heart of Germany. He finds that his knowledge of German is not sufficient to enable him to communicate freely with the people. He thinks in English, and has to go through a mental process of translation before he can understand what is said, or can himself say what he means. Constant communication with the people involves constant effort and a mental strain. Under such circumstances what a pleasure it is for him to meet with a person who can speak the English tongue. What a relief to be able to converse freely once more in his own vernacular. Words arise so spontaneously in the mind that the thought seems to evoke the proper expression.

But mark the result: the more he associates with English-

speaking people the less desire does he have to converse in German. The practice of the English language prevents progress in the aquisition of German. I have known of English people who have lived for twenty years in Germany without acquiring the language.

If our American school boy desires to become familiar with the German language, he must resolutely avoid the society of English-speaking people. He then finds that the mental effort involved in conversation becomes less and less, until, finally, he learns to think in German, and his difficulties cease.

Now consider the case of a deaf boy just graduated from an institution where the sign language has been employed as a means of communication. His vernacular is different from that of the people by whom he is surrounded. He thinks in the gesture language and has to go through a mental process of translation before he can understand what is said or written to him in English, and before he can himself speak or write in English what he desires to say. He finds himself in America, in the same condition as that of the American boy in Germany. If he avoids association with those who use the sign language, and courts the society of hearing persons, the mental effort involved in conversation becomes less and less, and finally he learns to think in English and his difficulties cease.

But such a course involves great determination and perseverance on the part of the deaf boy, and few, indeed, are those who succeed.

Not only do the other deaf-mutes in his locality have the same vernacular as his own, but they were his school fellows, and they have a common recollection of pleasant years of childhood spent in each other's society. Can it be wondered at, therefore, that the vast majority of the deaf graduates of our institutions keep up acquaintance with one another in adult life? The more they communicate with one another the less desire they have to associate with hearing persons, and the practice of the gesture language forms an obstacle to further progress in the acquisition of the English language.

These two causes (*a*) previous exclusive acquaintance with one another in the same school, and (*b*) a common knowledge of a form of language specially adapted for the communication of the deaf with the deaf, operate to attract together into the large cities large numbers of deaf persons, who form a sort of deaf community or society, having very little intercourse with the outside world.

They work at trades or businesses in these towns, and their leisure hours are spent almost exclusively in each other's society. Under such circumstances can we be surprised that the majority of these deaf persons marry deaf persons, and that we have as a result a small but necessarily increasing number of cases of hereditary deafness due to this cause. Such unions do not generally result in the production of deaf offspring, because the deafness of the parents in a large proportion of cases is of accidental origin, and accidental deafness is no more likely to be inherited than the accidental loss of a limb. Still I would submit that the constant selection of the deaf by the deaf in marriage is fraught with danger to the community.

Why the English Language should be Substituted for the Sign Language as a Vernacular.

If we examine the position in adult life of deaf children who have been taught to speak, or who have acquired the English language as a vernacular, whether in its written or spoken forms, we find an entirely different set of tendencies coming into play, especially if these persons have not been forced in childhood to make the acquaintance of large numbers of other deaf children, by social imprisonment for years together in the same school or institution apart from the hearing world.

Their vernacular use of the English language renders it easy for them to communicate with hearing persons by writing, or by word of mouth if they have been taught to articulate; and hearing persons can easily communicate with them by writing, or by word of mouth if they have been taught the use of the eye as a substitute for the ear. The restraints placed upon their intercourse with the world by their lack of hearing leads them to seek the society of books, and thus they tend to rise mentally to an ever higher and higher plane. A cultivated mind delights in the society of educated people, and their knowledge of passing events derived from newspapers forms an additional bond of union between them and the hearing world.

If they have formed in childhood few deaf acquaintances, they meet in after life hundreds of hearing persons for every deaf acquaintance, and if they marry, the chances are immensely in favor of their marrying hearing persons.

There is nothing in the deaf-mute societies in the large cities to

attract them, and much to repel them; for the more highly educated deaf-mutes in these societies speak what is to them a foreign language; while the greater number of the deaf-mutes to be found there are so ignorant that self-respect forbids them from mingling with them.

Thus the extent of their knowledge of the English language is the main determining cause of the congregation or separation of the deaf in adult life. A good vernacular knowledge of the English language operates to effect their absorption into society at large, and to weaken the bonds that tend to bring them together; whereas, a poor knowledge of the language of the country they live in causes them to be repelled by society and attracted by one another; and these attractive and repulsive tendencies are increased and intensified if they have been taught at school a language foreign to society and specially adapted for intercommunication among themselves. I say, then, let us banish the sign language from our schools. Let the teachers be careful in their intercourse with their pupils to use English and English alone. They can write, they can speak by word of mouth, they can spell the English words by a manual alphabet, and by any or all of these methods they can teach English to their pupils as a native tongue.

Conclusion.

In conclusion allow me to say:

1. That those whom we term "deaf-mutes" have no other natural defect than that of hearing. They are simply persons who are deaf from childhood and many of them are only "hard-of-hearing."

2. Deaf children are dumb, not on account of lack of hearing, but of lack of instruction. No one teaches them to speak.

3. A gesture language is developed by a deaf child at home, not because it is the only form of language that is natural to one in his condition, but because his parents and friends neglect to use the English language in his presence in a clearly visible form.

4. (*a*) The sign language of our institutions is an artificial and conventional language derived from pantomime.

(*b*) So far from being natural either to deaf or hearing persons, it is not understood by deaf children on their entrance to an institution. Nor do hearing persons become sufficiently familiar with the language to be thoroughly qualified as teachers until after one or more years' residence in an institution for the deaf and dumb.

(*c*) The practice of the sign language hinders the aquisition of the English language.

(*d*) It makes deaf-mutes associate together in adult life, and avoid the society of hearing people.

(*e*) It thus causes the intermarriage of deaf-mutes and the propagation of their physical defect.

5. Written words can be associated directly with the ideas they express, without the intervention of signs, and written English can be taught to deaf children by usage so as to become their vernacular.

6. A language can only be made vernacular by constant use as a means of communication, without translation.

7. Deaf children who are familiar with the English language in either its written or spoken forms can be taught to understand the utterances of their friends by watching the mouth.

8. The requisites to the art of speech-reading are:

(*a*) An eye trained to distinguish quickly those movements of the vocal organs that are visible (independently of the meaning of what is uttered.)

(*b*) A knowledge of *homophenes;* * that is, a knowledge of those words that present the same appearance to the eye; and

(*c*) Sufficient familiarity with the English language to enable the speech-reader to judge by context which word of a homophenous group is the word intended by the speaker.

If we look back upon the history of the education of the deaf, we see progress hindered at every stage by fallacies. Let us strive, by discussion and thought, to remove these fallacies from our minds so that we may see the deaf child in the condition that nature has given him to us. If we do this, I think we shall recognize the fact that the afflictions of his life are *mainly due to ourselves,* and we can remove them.

Nature has been kind to the deaf child, man cruel. Nature has inflicted upon the deaf child but one defect—imperfect hearing; man's neglect has made him dumb and forced him to invent a language which has separated him from the hearing world.

Let us, then, remove the afflictions that we ourselves have caused.

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* This word was suggested to me some years ago by Mr. Homer, lately Principal of the Providence (R. I.) School for Deaf-Mutes, and has now been permanently adopted.

1. Let us teach deaf children to think in English, by using English in their presence in a clearly visible form.

2. Let us teach them to speak by giving them instruction in the use of their vocal organs.

3. Let us teach them the use of the eye as a substitute for the ear in understanding the utterances of their friends.

4. Let us give them instruction in the ordinary branches of education by means of the English language.

5. And last, but not least, let us banish the sign language from our schools.

If it were our object to fit deaf children to live together in adult life and hold communication with the outside world as we hold communication with other nationalities than our own, then no better plan could be devised than to assist the development of a special language suitable for intercommunication among the deaf.

But if, on the other hand, it is our object to destroy the barriers that separate them from the outside world and take away the isolation of their lives, then I hold that our energies should be devoted to the acquisition of the English language as a vernacular in its spoken and written forms. With such an object in view we should bring the deaf together as little as possible and only for the purpose of instruction. After school hours we should separate the deaf children from one another to prevent the development of a special language and scatter them among hearing children and their friends in the outside world.

The subject being presented to the Society for discussion, Mr. E. M. GALLAUDET spoke, in substance, as follows:

I have listened with great interest to the remarks of Mr. Bell this evening, and am ready to agree in many particulars with the views he has so well presented.

I am, however, compelled to differ with him at several points; and as these involve matters of vital importance in the treatment of the deaf, I will beg the indulgence of the Society for a short time, while I attempt to show to what extent some of Mr. Bell's views are erroneous.

In proving the generally received opinion that the vocal organs of persons deaf from infancy are defective, to be a fallacy, Mr. Bell declared that difficulties encountered by such persons in acquiring speech are wholly external to themselves, and that all

persons so situated can, with proper instruction, be taught to speak and to understand the motions of the lips of others.

That this is a grave error has been proved by the experience of more than a century of oral teaching in Germany.

The late Moritz Hill, of Wessenfels, Prussia, a man of the widest experience and highest standing among the oral teachers of Europe, expressed to me the opinion a few years since that out of one hundred deaf-mutes, including the semi-mute and semi-deaf, only "eleven could converse readily with strangers on ordinary subjects" on leaving school. Of course a much larger number would be able to converse with their teachers, family, and intimate friends on common-place subjects; but it would be found that very many could never attain to any ready command of speech.

The explanation of this lies in the fact that a child, deaf from infancy, in order to succeed with speech and lip-reading must possess a certain quickness of vision, a power of perception, and a control over the muscles of the vocal organs, by no means common to all such children.

Mr. Bell's view has been held by many instructors with more or less tenacity, and *this* fact is explained by a readiness on their part to argue from the particular to the general. Having attained marked success with certain individuals, they draw, in their enthusiasm, the mistaken conclusion that success is possible in the case of every other deaf child, overlooking the fact that many things, besides the mere deafness of the child, may affect the result. Experience has demonstrated that in attempting to teach the deaf to speak, failure in many cases must be anticipated.

Mr. Bell is mistaken in supposing ignorance as to the mechanism of the vocal organs to be a prominent cause of failure to impart speech to the deaf. It is no doubt true that among persons unfamiliar with the training of the deaf, few have made the mechanism of speech a study; but in Germany, Italy, and France, not to speak of our own country, many are to be found who may be said to have mastered this subject. The results of their labors have been made available to instructors of the deaf, and all the best oral schools are profiting thereby.

Mr. Bell is also mistaken when he says that "in a majority of our schools and institutions articulation and speech-reading are taught to only a favored few, and in these schools no use of articulation is made as a means of communication," and that "few, if

any, attempts are made to teach articulation to those who have not naturally spoken." In most of the larger institutions for the deaf in this country, every pupil is afforded an opportunity to acquire speech, and instruction in this is discontinued only when success seems plainly unattainable.

It is a great error to suppose it to be true of a deaf person educated on what Mr. Bell calls the sign-method, that, "as a general rule, when his education is completed, his knowledge of the English language is like the knowledge of French or German possessed by the average hearing child on leaving school," or to say that "he cannot read an ordinary book intelligently without frequent recourse to a dictionary." On the contrary, a majority of persons thus educated have a good knowledge of their vernacular, are able to use it readily as a means of communication with hearing persons, and *are* able to read intelligently without frequent recourse to the dictionary.

When Mr. Bell has become familiar with the peculiarities of the deaf by personal contact with a large number of this class of persons, I am confident he will not repeat his assertion that "nature has inflicted upon the deaf child but one defect—imperfect hearing." For he will then have discovered, what has long been known to teachers of experience, that deaf children, in addition to their principal disability, are often found to be lacking in mental capacity, or in the imitative faculty, in the power of visual or tactile perception, and in other respects; all of which deficiencies, though they do not amount even to feeble-mindedness, much less to idiocy, do operate against the attainment of success in speech, as well as in other things which go to complete the education of such children.

Passing over several points of relatively small importance, in regard to which I believe Mr. Bell's views to be subject to criticism, I come to his characterization as a fallacy of the opinion held by many "that the language of gestures is the only language natural to the child born deaf or who has become deaf in infancy."

I think that in order to sustain his view that this is a fallacy Professor Bell gives a strained and very unusual meaning to the words "natural language." If, as he explains, a natural language is any one that a child may happen to be first taught by those with whom he is associated, then I should have no controversy with him. But I understand a natural language to be one that is mainly spontaneous, and not at all one that is borne in upon a child from without.

Moritz Hill, to whom I have already alluded, speaks of the language of signs as "one of the two universally intelligible innate forms of expression granted by God to mankind," the other being speech. Now it is hardly necessary to urge that speech is the form of expression natural to hearing persons, and I think a little reflection will satisfy most persons that with the deaf the language of signs is the only truly natural mode of expressing their thoughts.

Mr. Bell urges that the use of signs in the education of the deaf is a hinderance rather than a help, and that it would be better to banish them altogether. To this view I must give my earnest dissent.

I might, of course, cite the opinions of very many successful instructors of the deaf, who have followed only the sign method, to sustain my position, but I prefer to call in again the testimony of Moritz Hill, a man whose whole life was devoted to the instruction of the deaf by the oral method. In an exhaustive work on the education of the deaf,* Hill says, speaking of those who pretend that in the "German method" every species of pantomimic language is proscribed:

"Such an idea must be attributed to malevolence or to unpardonable levity. This pretence is contrary to nature and repugnant to the rules of educational science.

"If this system were put into execution the moral life, the intellectual development of the deaf and dumb, would be inhumanly hampered. It would be acting contrary to nature to forbid the deaf-mute a means of expression employed by even hearing and speaking persons. * * * It is nonsense to dream of depriving him of this means until he is in a position to express himself orally. * * * Even in teaching itself we cannot lay aside the language of gestures (with the exception of that which consists in artificial signs and in the manual alphabet—two elements proscribed by the German school), the language which the deaf-mute brings with him to school, and which ought to serve as a basis for his education. To banish the language of natural signs from the schoolroom and limit ourselves to articulation is like employing a gold key which does not fit the lock of the door we would open and refusing to use the iron one made for it. * * * At the best, it would be *drilling* the deaf-mute, but not *moulding* him intellectually or morally."

* Der gegenwärtige Zustand des Taubstummen Bildungswesens in Deutschland; von Hill, Inspector der Taubstummen Anstalt zu Wetssenfels; Ritter des St. Olafs, &c. Weimar, H. Böhlau, 1866.

Hill then follows with thirteen carefully formulated reasons why the use of signs is important and even indispensable in the education of the deaf.

Mr. Bell is in error when he supposes that in the so-called sign-schools verbal language is only imparted through the intervention of the sign-language. In many well-ordered schools of this class, language is taught without the use of signs, and in such schools the language of signs is kept in its proper position of subordination. It goes without saying that in schools for the deaf there may be an injudicious and excessive use of signs. This is always to be guarded against, and when it is, I am convinced that no harm, but great good, results from the use of signs in teaching the deaf.

Furthermore, it is well known that the attempt to banish signs from a school for the deaf rarely succeeds. Miss Sarah Porter, for three years an instructor in the Clarke Institution at Northampton, Mass., an oral school in which most excellent results have been attained, shows candor as well as judgment when she says, in a recent article in the *American Annals of the Deaf and Dumb*, "Every oral teacher knows that fighting signs is like fighting original sin. Put deaf children together and they will make signs secretly if not openly in their intercourse with each other."

It is not true as a matter of fact that the use of signs necessarily prevents the deaf from acquiring an idiomatic use of verbal language and from thinking in such language. Large numbers of them who have never been taught orally have come into such a use of verbal language, and while it is granted that many educated under the sign system do not use verbal language freely and correctly, the same is found to be true of very many who have been educated entirely in oral schools.

In one important particular the language of signs performs a most valuable service for the deaf, and one of which nothing has yet been found to take the place. Through signs large numbers of deaf persons can be addressed, their minds and hearts being moved as those of hearing persons are by public speaking in its various forms.

Having seen the good effects on the deaf of the discreet use of the sign-language through a period of many years, I am confident that its banishment from all schools for the deaf would work great injury to this class of persons intellectually, socially, and morally.

The Hon. GARDINER G. HUBBARD being present, was invited by the chair to participate in the discussion. He said he had been connected with the Clark Institution for many years. The deaf pupils in that school are taught entirely by articulation.

From recent inquiries which had been made to ascertain how far the graduates had profited by instruction in articulation, it appeared that in almost in every instance they could carry on conversation with others sufficiently to engage in many kinds of business from which they would have been excluded if they had only used signs.

It was true, as Mr. Gallaudet said, the congenitally deaf were frequenty able to articulate more distinctly than those who lost their hearing at an early age, but this arises from the fact that the disease that caused the deafness affected the organs of articulation to a greater or less degree; but the congenitally deaf do not make as rapid progress in their studies as those who had once spoken, for these have a knowledge of language which the former could obtain only by long protracted study.

Mr. Hubbard believed that the pupils at the Clark Institution made at least as rapid progress in all their studies as those taught by signs; while, at the same time, they acquired the power of reading from the lips and speaking, in which those taught by signs were deficient.

When the first application was made to the Legislature of Massachusetts for the incorporation of the Clark Institution, Mr. Dudley, of Northampton, chairman of the committee to whom the petition was referred, had a congenitally deaf child under instruction at Hartford. The petitioners were opposed by the professors from the asylum, as they believed an articulating school would retard the education of the deaf, as it was impractical to teach the deaf by articulation, that system having been tried and proved a failure, and the new method was stigmatized as one of the visionary theories of Dr. Howe, (the principal of the Perkins Institute for the Blind, and the teacher of Laura Bridgeman, the blind deaf mute,) who was associated with the petitioners in the hearing.

The application was rejected through the influence of these professors and of Mr. Dudley, who 'knew, from experience with his own child, that it was impossible to teach the congenitally deaf to talk.'

Two years after, our application was renewed and with better success.

Mr. Hubbard in the meantime, with the aid of Miss Rogers, had opened a small school where the deaf were taught to speak. This school was visited and examined by the committee, and the progress made was so great that Mr. Dudley became a warm convert, convinced that the impossible was possible, and the application was granted, although again opposed by the gentlemen from Hartford. The school was opened at Northampton, and has been in operation for nearly fifteen years, and teaching by articulation has ceased to be a visionary theory.

Many of the warmest friends of the Institution now are, like Mr. Gallaudet, connected with institutions where signs are used. In almost every institution for the deaf classes are now taught to articulate, though articulation is not used as the instrument for instruction.

Mr. Gallaudet had taken exception to the remark of Mr. Bell, that idiots were born dumb, and said that in every school for idiots there were many feeble-minded children who could talk readily; but Mr. Bell used the word idiot not as simply a feeble-minded person, but according to its ordinary meaning, "a human being destitute of reason or the ordinary intellectual powers of man."

It has always been the policy at Northampton to prevent, as far as possible, marriages of deaf with deaf, for the records show that the children of such intermarriages are often deaf; and even where a congenitally deaf person marries a hearing person, the children sometimes are deaf.

The tendency of the intermarriage of the deaf would be to raise a deaf race in our midst.

About one in 1,500 of the population are deaf; but if these intermarriages should take place and a deaf race be created, the proportion would rapidly increase. The object of all friends of the deaf should be to prevent the deaf from congregating, and to induce them to associate with hearing people. In bringing the deaf together in institutions, where they are taught by signs, the tendency is to make the deaf deafer and the dumb more dumb.

It was originally intended to have only a family or small school at Northampton, but it was soon found that signs could not be excluded from the play-ground, as the young children could not communicate in any other way. The plan was changed, the number of pupils was largely increased, and a preparatory department established, in which signs were tolerated on the play-ground. On

the removal of the pupils to the higher departments, the use of signs is forbidden, and they are rarely used on the play-ground or between the pupils, either in or out of school hours.

In the later years of instruction they acquire great facility in articulation and reading from the lips, though there is almost always some difficulty for a stranger to understand them.

Mr. Gallaudet had referred to the International Convention of deaf-mute teachers and their friends, at Milan, three years ago. Mr. Hubbard was present at the convention held this year at Brussels, and was there informed that a delegate had been sent from France to attend the convention at Milan and investigate the method of instruction in Italy, where articulation was used, for the purpose of deciding whether the instruction in the French schools should continue to be by signs, or instruction by articulation be substituted for signs.

The preference of the delegate had been for signs, but on witnessing the results obtained in the Italian schools and hearing the discussion, he was led to advise that the instruction in the French schools hereafter be by articulation, instead of signs, and such a change has, Mr. Hubbard understands, been made in most of the schools of France.

Mr. Hubbard learned from the reports at Brussels that almost all the European schools were taught by articulation, and that this means of instruction was being rapidly substituted for the sign language in England as well as in France.

Mr. Bell, in reply to the remarks of Mr. Gallaudet, said:

There are signs *and* signs. There is the same distinction between pantomime and the sign-language that there is between a picture and the Egyptian hieroglyphics.

Pictures are naturally understood by all the world, but it would be illogical to argue from this that a picture-language, like that developed by the ancient Egyptians, must also be universally intelligible. Pantomime is understood by all the world, but who among us can understand the sign-language of the deaf and dumb without much instruction and practice?

No one can deny that pantomime and dramatic action can be used, and with perfect propriety, to illustrate English expressions so as actually to facilitate the acquisition of our language by the deaf; but the abbreviated and conventionalized pantomime, known

as the "sign-language," is used *in place* of the English language, and becomes itself the vernacular of the deaf child.

Judging from the quotations given by Dr. Gallaudet, Moritz Hill himself makes a clear distinction between pantomime and the sign-language, retaining the former and proscribing the latter. "Every species of pantomimic language is not proscribed," he says. "Natural signs," or "signs employed by hearing and speaking persons," are retained, while "artificial signs" are proscribed.

All the arguments that have been advanced regarding pantomime and a pantomime language are equally applicable to pictures and a picture-language. For instance, we may say that a picture-language is more natural than any of the spoken languages of the world, because pictures are naturally understood by all mankind. We may even arrive, by a further process of generalization, at the idea that picture-language, in the wider sense, really constitutes the only form of language that is natural at all, for all the other languages of the world appear to be entirely arbitrary and conventional. If we pursue the parallel we shall arrive at the conclusion that a picture-language of some kind must necessarily become the vernacular of our pupils, through which the other more conventional languages may be explained and taught.

It is immaterial whether such statements are fallacious or not, so long as we do not apply them to educational purposes. But let us see how they work in practice. The exhibition of a picture undoubtedly adds interest to the fairy tale or story that we tell a child. It illustrates the language we use, and it may be of invaluable assistance to him in realizing our meaning. But is that any reason why we should teach him Egyptian hieroglyphics? Granting the premises: Is the conclusion sound that we should therefore teach him English *by means* of hieroglyphics?

If such conclusions are illogical, then the fundamental ideas upon which our whole system of education by signs is based are also fallacious and unsound.

One word in conclusion regarding speech.

The main cause of the fallacies that fog our conception of the condition of the deaf child is *his lack of speech.* A deaf person who speaks is regarded by the public more as a foreigner than as a deaf mute. Speech, however imperfect, breaks through the barriers of prejudice that separate him from the world, and he is recognized *as one of ourselves.*

Mr. Gallaudet under-estimates the value of speech to a deaf child. He seems to think that speech is of little or no use, unless it is as perfect as our own. The fact is that the value of speech to a deaf child must be measured by its *intelligibility* rather than by its perfection.

It is astonishing how imperfect speech may be and yet be intelligible. We may substitute a mere indefinite murmur of the voice for all our vowel sounds, without loss of intelligibility. (Here Mr. Bell spoke a few sentences in this way, and was perfectly understood.) Here at once we get rid of the most difficult elements we are called upon to teach. If now we examine the relative frequency of the consonantal elements, we shall find that 75 per cent. of the consonants we use are formed by the point of the tongue, and that the majority of the remainder are formed by the lips. The consonants that are difficult to teach are chiefly formed by the top or back part of the tongue; but, on account of their comparative rarity of occurrence, they may be very imperfectly articulated without loss of intelligibility. Hence I see no reason why, in spite of the general ignorance of teachers respecting the mechanism of speech, we may not hope to teach all deaf children an intelligible pronunciation.

Let teachers appreciate the value of intelligible speech to a deaf child, and they will make the attempt to give it to him. At the present time, lack of appreciation operates to prevent the attempt from being made upon a large scale. Skilled teachers of articulation will become more numerous as the demand for their service increases, and their ingenuity, intelligently applied, will increase the perfection of the artificial speech obtained.

In the meantime, do not let us discard speech from the difficulty of obtaining it in perfection. Do not let us be misled by the idea that intelligible but defective speech is of no use, and must necessarily be painful and disagreeable to all who hear it. Those who have seen the tears of joy shed by a mother over the first utterances of her deaf child will tell you a different tale. None but a parent can fully appreciate how sweet and pleasant may be the imperfect articulation of a deaf child.

240TH MEETING. NOVEMBER 10, 1883.

The President in the chair.

Forty-eight members present.

Announcement was made of the election to membership of Ethelbert Carroll Morgan.

It was announced from the General Committee that invitation had been extended to the members of the Anthropological and Biological Societies to attend the meeting of December 8th, for the purpose of listening to the annual address of the President.

Mr. Edwin Smith exhibited a

SEISMOGRAPHIC RECORD OBTAINED IN JAPAN,

describing the apparatus by which it was made, and giving a brief account of the seismographic investigations of Professor J. A. Ewing.

Remarks were made by Mr. Antisell.

Mr. C. E. Dutton made a communication, entitled

THE VOLCANIC PROBLEM STATED.

[Abstract.]

It is sufficiently obvious that the volcano is a heat problem, or a thermo-dynamic problem. All volcanic activity is attended with manifestations of great energy. This energy is due to the elastic force of considerable quantities of water occluded in red-hot or yellow-hot lavas. The problem is to find a satisfactory explanation of the origin of the heat, the origin of the occluded water, and their modes of reaction.

In attempting this solution, various explanations have been conjectured. The first to be noticed, and the one which, in various forms, has met with the most favor from geologists and physicists, is that the source of heat is primordial—*i. e.*, it is the remains of a large amount of heat contained by the entire earth-mass in its supposed primordial condition, according to the nebular hypothesis; that water has penetrated from above, either from the ocean or from lakes; and that the contact of cold water with the hot magmas within the earth is a summary explanation of the phenomena. This view is supported by the following considerations: 1st, the contact of water with intensely hot bodies and the resulting generation of great explosive force is matter of the commonest experience; 2d, the outer rocks and strata are known to be full of fissures, and the ocean bottom and lake bottoms are, therefore, presumably very leaky; 3d, nearly all active volcanoes are situated either within, or

in the neighborhood of, large bodies of water; 4th, volcanoes near the sea often deliver salts which may reasonably be supposed to be the same as those contained in the ocean; 5th, the analogy of geysers gives us a series of phenomena which seem to be, in many respects, quite parallel, and which have been satisfactorily explained in a similar way.

To this view of the origin and causation of volcanic activity there are some objections. There is difficulty in understanding how water obtains access to hot magmas. No doubt the rocks are full of fissures, but we cannot, by any means, confidently infer that these fissures extend sufficiently deep to afford free or even capillary passages to melted magmas beneath. We should more legitimately infer that the heat increases gradually with the depth. At a depth of a few miles the rocks presumably have a temperature which, though high, is still below fusion, and at such temperatures it is well known that all the siliceous or rocky materials we are acquainted with are viscous. Remembering the immense statical pressure due to a thickness of a single mile of rocks, all fissures at such depths would be closed, as if the rocks were wax or butter.

2d. Although the contact of cold water with intensely hot masses will surely produce a violent explosion, we are not at liberty to admit offhand that cold water does obtain such contact in the volcanoes. On the contrary, as it penetrates it takes up the heat of the rocks through which it passes. But water is believed by all physicists to have what is technically termed a critical temperature, *i. e.*, a temperature at which it can exist only in the form of vapor however great the pressure, and this temperature is computed theoretically to be about 772° F., which is far below that of melted rock. If therefore, water could reach the liquid lavas below, it would reach them only in the form of vapor. There is indeed no difficulty in supposing that the vapor of water may, under great statical pressure, be forced into the rocks, passing between inter-molecular spaces. This is but one aspect of the phenomena of the diffusion and occlusion of gases in solids, and we know that water-vapor in large quantities is readily occluded by lava. But this is evidently no explanation of the explosive action. It is in the broadest possible contrast with the gross conception of the sudden access of cold water to hot bodies. The presumption is, under the process here suggested, that the vapor of water might penetrate slowly into regions of great heat until the hot magmas were saturated, and then the

process would come to a standstill. But there would be no volcano in this case, for the supposed condition is evidently statical and stable. For the pressure which is supposed to force the vapor in is that due to the hydrostatic pressure of a column of water. The pressure which keeps it from blowing out is that due to an equally high or even higher column of rock, the density of which is at least two and a half times greater.

3d. The analogy of the geyser thus fails to become a true homology, or an epitome of the volcano. For the geyser is due to the access of cold water to a cavity walled by hot rocks and its vaporization ; the volcano, if due to the penetration of water, is due to penetration in the form of vapor in the first instance; and the difference is radical.

4th. The proximity of volcanoes to large bodies of water does not necessarily imply a logical and causal relation, and is not necessarily the true law of distribution. Another and perhaps a more rational law of distribution may be given. As a matter of fact all active volcanoes are not situated near seas or lakes, though in truth the exceptions are at the present time few, as for instance, Sangay, in the eastern Cordilleras of Peru, and the volcanoes of Central Asia. It seems as if Darwin had acutely divined the true association, viz: that volcanoes are situated in areas which are undergoing elevation. So far as we know this rule is without exception, but there are many cases where the verification of the elevation is wanting. So far, however, as the test has hitherto been applied it has approved the rule. This is especially conspicuous in the western half of our own country when applied to the late Tertiary and Post Tertiary volcanoes, and it is true, so far as known, of the Andes, Java, Phillippines, and Mediterranean, and I have recently been able to verify it in the case of the Hawaiian volcanoes. It happens that elevations, as well as subsidences, are much more frequent and extensive near coast lines than in continental interiors, whence the proximity of volcanoes to the sea becomes a secondary rather than a primary relation. But elevations also occur in continental interiors, though less frequently. And when they do occur, we find associated phenomena of volcanism as abundant and forcible as in littoral regions. This has been the case in the great Tertiary elevation of the Rocky Mountains, of the Alps, and of the Himalayan plateau. Darwin's law of the distribution of volcanoes is as thoroughly sustained by geological history as by modern instances;

while the other law, though largely predominant at the present period, shows a few conspicuous failures at the present time, but a very large number of them in times past.

Another hypothesis to account for volcanic energy supposes the interior of the earth to consist of unoxidized elements, which gradually become oxidized by the penetration of oxygen from the atmosphere.

The objections to this hypothesis are as follows: On the assumption that the earth acquires no oxygen from space, the primitive atmosphere would have been many thousand times greater than at present; but the geological record argues strongly in favor of an atmosphere which may indeed have varied in quantity and composition, but nowhere near so greatly as the hypothesis implies. Any such extravagant difference would have recorded itself legibly in the strata. Furthermore, on this view, the end of all volcanic activity is close at hand. Only three pounds of oxygen to the square inch of terrestrial surface are left. A few hundred or thousand centuries and the last volcanic beacon is extinguished, and with it all organic life.

But suppose the earth gathers up oxygen in its march through space. This may be true, but we can make any supposition on this point which pleases our fancy and feel sure that no prudent scientific man will dispute it.

A third hypothesis is that of the late Robert Mallet, which assumes the earth to be contracting interiorly by a secular loss of primitive heat. As the interior cools and shrinks, the external shell is crushed and crumpled together, and this mechanical crushing is a sufficient source of heat.

To this hypothesis there are many answers. The most direct one is that the very facts which are relied upon to prove that there is any interior cooling at all now going on also prove that the amount hitherto has been excedingly small, and has been limited as yet to a thin external shell, not exceeding 150 miles in thickness, while the great interior is about as hot as ever; but, by the terms of the hypothesis, if the interior has not cooled there has been no interior contraction. The hypothesis is refuted by taking its own premises and pushing them to their inevitable conclusions.

There is a fourth hypothesis, which cuts the Gordian knot instead of untying it. It assumes, as the result of causes unexplained, heat is generated locally within the earth, and such local movements

of heat are the cause of volcanism. This is an arbitrary postulate, which, by its own terms, precludes discussion. Nevertheless it is the one which I believe agrees best, and perhaps perfectly, with observed facts. It undoubtedly sweeps away the difficulties which encumber all other hypotheses, but unfortunately it is an appeal to mystery, and therefore substitutes a single difficulty as great as, if not greater, than all the other difficulties put together.

There is a fifth hypothesis, which takes account of the fact that many bodies which are solid under great pressure are immediately liquefied when the pressure is removed, heat being neither lost nor gained. The removal of pressure by denudation of the surface above the seat of lavas may thus determine volcanic action. The reply to this is that volcanoes do not always, nor even generally, occur where such denudation and consequent relief of pressure, are in progress. The true law of the distribution of volcanoes appears to be the one given by the late Charles Darwin, viz., that they occur in areas which are undergoing elevation.

There are several broad facts, or categories of facts, which a true theory of the volcano must cover, and which will be recited briefly.

1. Lavas, in their subterranean seat, could not possibly have been in a highly elastic explosive condition from the earliest epochs of the earth's evolution, and only waiting a convenient season to break forth. We have no alternative but to regard them as being inert and inexplosive in their primitive condition, and as having acquired explosive energy just before the epoch of eruption. To assume that they have always been in the condition they present while pouring forth, and that the opening of a fissure has been the accident which determined the eruption, is reasoning in a circle. It is the energy of the lavas which causes the fissure, and not the fissure which causes the lavas to extrude. The lavas extrude themselves by virtue of their acquired elastic force. The theory must explain how materials which antecedently were inert, passive, incapable of eruption, may become active, dynamical, eruptible.

2. Another broad fact, closely related to the foregoing, is the intermittent action of volcanoes. These vents do not discharge all their available products at once, but by repeated spasms of activity, separated by longer intervals of repose. If these fiery explosive liquids had lain so long in the earth, chock-full of energy and only awaiting the opening of a passage-way, how happens it that when

a vent *is* once opened they do not all rush forth at once, and continue to outpour until the reservoir is completely exhausted, and why does not the vent thereafter close up forever? In a word, why should a volcano dole out its products in driblets, instead of sending forth one stupendous belch, equal to all the driblets combined? The answer here proposed is that it is because lavas, in their primitive condition, do not have sufficient potential energy, in the form of elastic force, to break open the covering which keeps them in; but they gradually acquire that energy in a portion of the reservoirs at a time, and when a sufficient portion of them has acquired it the covering is ruptured, and the whole of this energetic portion is extravasated. The vent then closes, and the process is repeated upon a second installment. The agency which thus progressively develops this force is the missing factor, and when we discover it we shall discover the secret of the volcano.

The third general fact to be taken account of is the enormous quantity of heat given off by volcanoes through long periods of time without any sign of exhaustion. The quantity of heat brought up by the lavas themselves is but a fraction of the whole amount dissipated. Kilauea wastes many times more heat by quiet radiation from the surfaces of its lava lakes and by steaming and by numberless modes of escape than by actual eruption of lavas. Mauna Loa also dissipates the greater part of its heat in the same way, and the same fact is wholly or partially true of all other active or intermittent volcanoes. And yet for very long periods, for thousands of centuries, these great volcanoes show no sign of heat-exhaustion; on the contrary, such indications as we have suggest the conclusion that the earth beneath them is hotter than before.

A fourth general fact is that volcanoes are located in areas which have recently been or are now undergoing elevation.

All these facts suggest the action of some cause generating heat within the earth. This cause, if such it be, is for the present wholly mysterious and unknown.

Mr. Powell, referring to the relation between volcanic eruption and elevation, said that the typical, secular sequence of geologic events was, first, elevation, resulting in, second, degredation, accompanied by, third, extravasation, followed sooner or later by, fourth, subsidence, resulting in, fifth, sedimentation. There are numerous regions in which this circle of events has been recorded, and in some places it has been repeated two or three times.

Mr. F. W. CLARKE suggested that the difficulty in the way of a chemical explanation of volcanic phenomena was due to our ignorance of chemical force under high pressures. Spring has lately shown that chemical union could be brought about by pressure alone. Hence, water coming in contact with molten rock matter in the interior of the earth might be prevented from dissociating. If, however, dissociation takes place, we may conceive that water may play the following part in volcanic explosions. Gradually filtering through the surface rocks to the hot lava, it would undergo slow decomposition, and great quantities of mixed oxygen and hydrogen would thus slowly accumulate. Now let a process of cooling begin. Soon the temperature at which oxygen and hydrogen unite would be reached, and explosive union would occur. This may account for volcanic explosions, at least in part. By such a process, potential energy is gradually stored up, to be later, suddenly or instantaneously, released. This hypothesis does not account for volcanic heat, but presupposes its existence.

Mr. WHITE, referring to Mr. Powell's remarks on the instability of continental areas, said that the prevalent doctrine of the permanence of oceans, and the gradual development of the continents, was not sustained by paleontology. Continents were needed somewhere to develop the land plants and land mammals which appeared during the emergence of the known continents.

Mr. HARKNESS pointed out that Mr. White was postulating unknown continents to support the Darwinian hypothesis, to which Mr. White assented.

Mr. POWELL added, that in detailing the great cycle of geologic events, he should have included metamorphism as a sixth term, resulting from burial by sediment; and Mr. DUTTON remarked that he had included this consideration in a paragraph contained in his written manuscript, but not read.

Mr. McGEE made a communication on

THE DRAINAGE SYSTEM AND THE DISTRIBUTION OF THE LOESS OF EASTERN IOWA.

[Abstract.]

The most conspicuous geographic feature of eastern Iowa is the remarkable parallelism among its water-ways. Yet the region comprises two essentially distinct geologic tracts; and the coincidence

in direction of drainage in these is fortuitous: 1. The Wisconsin Driftless Region so far extends into the northeastern corner of Iowa as to include all of the triangular area bounded on the southwest by the elevated Niagara escarpment extending from the extreme eastward projection of the state northwestwardly to the Minnesota line, fifty miles west of the Mississippi. Within this tract, the drainage was originally determined by general surface slope and by rock-structure, and the present topography, which is varied and picturesque, was developed by sub-ærial erosion. 2. Within the far more extensive tract formed by the glacial drift and its derivatives, the surface is a gently undulating plain, over which the general relief is inconspicuous, and the local topography faintly defined though singularly uniform and symmetric in character; and here the parallelism in drainage is prevalent and characteristic. There are, indeed, both local and general exceptions to this parallelism, which exemplify a variety of types of aberrant behavior of the streams; but while these impair the geographic symmetry of the drainage system, they add much more largely to its geologic significance. Putting together the instances of accordant, and neglecting the instances of aberrant extension of water-lines, a *normal direction of drainage* for the whole of the drift-formed tract might be empirically determined; which normal direction is represented by a symmetric series of slightly divergent and slightly curved lines, concave to the northeastward, radiating from a point north of the state in a general southeasterly direction, toward the Mississippi. Probably nowhere else on the surface of the globe does so symmetric a normal drainage system exist, and assuredly nowhere else does the sum of directions of stream-flow over so considerable an area present so few examples of departure from the normal.

The broader topographic features of eastern Iowa are dependant upon geologic structure. The dip of the rocks is to the southwest, and the outcrops of the several formations represented form successive approximately parallel zones (trending northwest and southeast), of which those of the Niagara and Hamilton are widest. Now the Niagara rocks resisted well the planation of the pre-quaternary eons, and their eastern margin is accordingly defined by a prominent escarpment varying from 1,000 to 1,350 feet in altitude, from which there is a steep northeasterly slope to the Mississippi, and a gentle inclination, corresponding to the dip of the strata, in the opposite direction. The Hamilton rocks, on the other hand, have so

yielded to erosion that their area is topographically represented by a broad, shallow trough, of which the altitude is only from 600 to 1,000 feet, and of which the sides rise and culminate in the Niagara escarpment on the east and in the Mississippi-Missouri water-shed on the west. There is, however, a subordinate general topographic feature which is independent of geologic structure. A wide, gentle, indefinitely outlined depression extends directly across the great eastward projection (the "Cromwell's Nose") of Iowa and diagonally across the Upper Silurian, Devonian, and Carboniferous rocks alike, in the line of the general course of the Mississippi, from near the mouth of the Turkey to the mouth of the Iowa. It is manifestly of great antiquity.

Thus, in its general topography, eastern Iowa is characterized, primarily, by an elevated escarpment near its eastern border, by a broad depression intersecting its western portion diagonally, and by a general southwesterly slope extending over most of its area; and secondarily, by an indefinite ancient valley cutting off its eastern projection. And its general drainage system is almost absolutely independent of this general topography; for not only do the principal streams flow at right angles to the prevailing slope and cut through the elevated escarpment when it lies in their way, but, with the single exception of the Cedar, they preserve their courses directly across the ancient valley.

In their relation to minor topographic features the rivers of eastern Iowa conform to two diametrically opposite laws: 1. for two-thirds or three-fourths of their combined length they flow in the axes of the ill-defined, shallow valleys which characterize the drift-plain; and, 2, for the remaining portion of their courses they flow in narrow gorges which they have excavated for themselves in the axes of the elongated ridges that constitute the leading features in the local topography of the region. Moreover, they have in many instances, at the same time gone out of their direct courses, and deserted valleys already prepared for them, to attain the anomalous positions assumed under the second law of association. And let it be noted that in every such case the gorges have demonstrably been carved by the streams themselves through the quaternary and older formations alike; that the pre-existent valleys which they avoided have not been appreciably eroded since the quaternary; and that there has been no localized orographic movement in the region since long antecedent to the quaternary.

The principal tributaries entering the rivers from the right similarly conform to two antagonistic laws in their relation to topography: 1. Most of them flow throughout their courses in directions coincident with local and general slopes, and avoid elevations in their vicinity; and, 2. Many of them originate with directions approaching those normal to their localities, but curve more and more to the left toward their mouths, until they flow directly against the general slope, and enter the rivers at large angles; and all such streams have high north banks which they closely hug, and low south banks which they avoid.

So the drainage system of eastern Iowa is essentially independent of the more general topographic features, though affected by local topography; and the relations of the waterways to local topography are largely anomalous, and without parallel elsewhere.

Though essentially continuous stratigraphically, and of unquestionable genetic unity, the loess of eastern Iowa is variable in many characters, and may be separated into three geographic divisions; viz: 1, the Driftless Region division; 2, the Riparian division; and, 3, the Southern division. That of the first division forms the surface throughout the Driftless Region, as it exists in Iowa, and everywhere overlaps the eastern border of the drift; it is generally rather coarse, heterogeneous, and non-calcareous, and yields depauperate fossils of characteristic species; it reposes upon or graduates into a thin stratum of water-worn erratic materials, which, in turn, rests upon either the residuary clays of the Driftless Region or the margin of the drift-sheet; its western border is exceedingly sinuous, affects the greatest altitudes, and invariably overlooks the contiguous drift-plain; and, in capping the elevated Niagara escarpment, it forms the highest land within hundreds of miles, except in northerly directions. The loess of the Riparian division occurs chiefly in the elongated ridges so common and so intimately associated with the waterways in eastern-central Iowa; it is often fossiliferous, and its characters are generally typical; it usually graduates downward into stratified sands or gravels, which may or may not merge into drift; and it invariably seeks the highest summits in the region;—for the ridges in which the rivers have carved their cañons are always loess-topped; wherever streams avoid low-lying valleys for high-lying plateaus, the plateaus are of loess exteriorly; and the high northern banks of the aberrant tributaries are generally loess-capped. The loess of the Southern division prevails

over southeastern Iowa; it abounds in characteristic fossils (which may or may not be depauperate), in loess-kindchen, and in calcareous tubes; it is fine, homogeneous, and vertically cleft; it generally graduates into the subjacent drift so imperceptably that neither geographic nor stratigraphic separation of the formations, by other than a purely arbitrary line, is possible; and it occurs indiscriminately at all levels.

So, in its distribution, the loess of eastern Iowa is intimately connected with the Driftless Region, with the drainage, and with the topographic configuration; but in its disposition to seek the greatest altitudes in the north, and to merge into the drift in the south, its behavior is as anomalous as is that of the rivers of the same region.

Mr. Powell remarked that these peculiarities of drainage were different from those observed in the drainage systems of mountain regions and demanded a different explanation, which was not yet forthcoming. It was probable, however, that not enough allowance was made for the differential effects of general degradation subsequent to the determination of the drainage.

Mr. Gilbert, after defining antecedent and super-imposed drainage, said that Mr. McGee's description definitely negatived the hypothesis of antecedent drainage, and rendered the hypothesis of super-imposed drainage in the ordinary sense equally untenable. The most plausible alternative is the hypothesis suggested by Mr. McGee in one of his earlier papers, that the drainage was super-imposed by the ice-sheet, the distribution of loess having been determined at the same time and by the same causes.

Mr. White regretted that Mr. McGee's special investigations did not include the portion of Iowa draining to the Missouri. The details of drainage in that region are equally interesting, and, in his opinion, do not admit of the explanation mentioned by Mr. Gilbert. The direction of the rivers diverges at right angles from that of the Mississippi tributaries, and their valleys are excavated from loess except along their upper courses.

Mr. Powell said that on the Illinois side of the Mississippi River many of the features described in the paper are repeated. The loess hills follow the river courses, and in the opposite directions overlook plains. The explanation of the phenomena is problematic, but the theory advocated by Mr. Gilbert does not appear sufficient.

241st Meeting. November 24, 1883.

Vice-President Billings in the Chair.

Fifty-three members and guests present.

It was announced by the Chair that the next meeting would be held in the Lecture Hall of the National Museum, that the members of the Anthropological and Biological Societies were invited to be present, and that the members of all three societies were requested to invite their friends.

Opportunity was afforded for the introduction of amendments to the Constitution, but none were offered.

Mr. C. D. Walcott made a communication on

THE CAMBRIAN SYSTEM IN THE UNITED STATES AND CANADA.

[Abstract.]

Defining the Paleozoic period as has been done by Geikie in his Text-Book of Geology, it will include all the older sedimentary formations containing organic remains, up to the top of the Permian. Upon the paleontologic evidence it may be divided into an "older and newer division, the former (from the base of the Cambrian to the top of the Silurian system) distinguished more especially by the abundance of its graptolitic, trilobitic, and brachiopodous fauna, and by the absence of vertebrate remains; the latter (from the top of the Silurian system to the top of the Permian system) by the number and variety of its fishes and amphibians, the disappearance of graptolites and trilobites, and the abundance of its cryptogamic terrestrial flora." The two divisions may be still further subdivided; the upper into the Carboniferous and Devonian, the lower into the Silurian above and the Cambrian beneath. It is the Cambrian division we now have to consider.

Stratigraphically it is difficult to fix any definite upper limit to the Cambrian system, owing to local causes having affected the conditions of sedimentation and consequent extinction or continuance of the fauna. Upon the evidence of the section in New York State on the western side of Lake Champlain, the Potsdam sandstone closes the period stratigraphically and paleontologically, the Calciferous formation forming little more than a closing deposit of the Potsdam; and the large Chazy fauna appearing suddenly in the overlying limestone is entirely distinct from that of the Potsdam. In central

Nevada the section passes through limestones marked by the presence of a typical Potsdam fauna and on up to one that has the general facies of that of the Trenton Lower Silurian fauna. Midway of these passage beds occur layers of rock that carry representatives of both the Cambrian and Silurian faunas. Above this band the Cambrian fauna gradually disappears, and below it soon predominates to the exclusion of the Silurian types. In this section we have an illustration of the gradual extinction of an older fauna as a new one is introduced, the sedimentation continuing and no physical disturbance occurring to change the conditions necessary for the presence of animal life. It is the ideal section uniting the faunas of two periods, and if we had the blanks filled in between all the groups, as the blank between the Potsdam and Chazy in New York is filled in by the Nevada section, the Paleozoic would be a record of continuous connected organic life from the base of the Cambrian to the summit of the Permian.

It is convenient for stratigraphic geologic work to separate the Paleozoic series into subdivisions, and, as this is almost necessarily done on paleontologic evidence, I would separate the Cambrian as one characterized by what Barrande has named the first fauna.* Applying this to the Nevada section already mentioned, the line between the Cambrian and Silurian would be drawn where the types of the second fauna begin to predominate. With this definition of the Cambrian system, the strata referred to it in the United States and Canada will be briefly noticed.

In the Grand Cañon of the Colorado the top of the Cambrian is the Tonto formation, a series of sandy calcareous strata 1,000 feet in thickness. The contained fauna is closely allied to that of the Potsdam sandstone and continues up to the summit of the formation, the overlying Devonian rocks resting directly above strata containing *Lingulepis*, *Iphidea*, *Conocephalites*, *Dicellocephalus*, etc. The Tonto rests uncomfortably on strata that were extensively eroded prior its deposition. This lower series comprises over 11,000 feet of unmetamorphosed shales, limestones, and sandstones, with 1,000 feet of interbedded lavas. It forms the Grand Cañon and Chu-ar′ groups of Powell and is characterized by the presence of a few fossils that enable us to refer it to the Cambrian but not to define its stratigraphic horizon. That is done on the evidence of the position it occupies with reference to the Tonto.

* The paleontologic evidence and discussion will appear in a future paper.

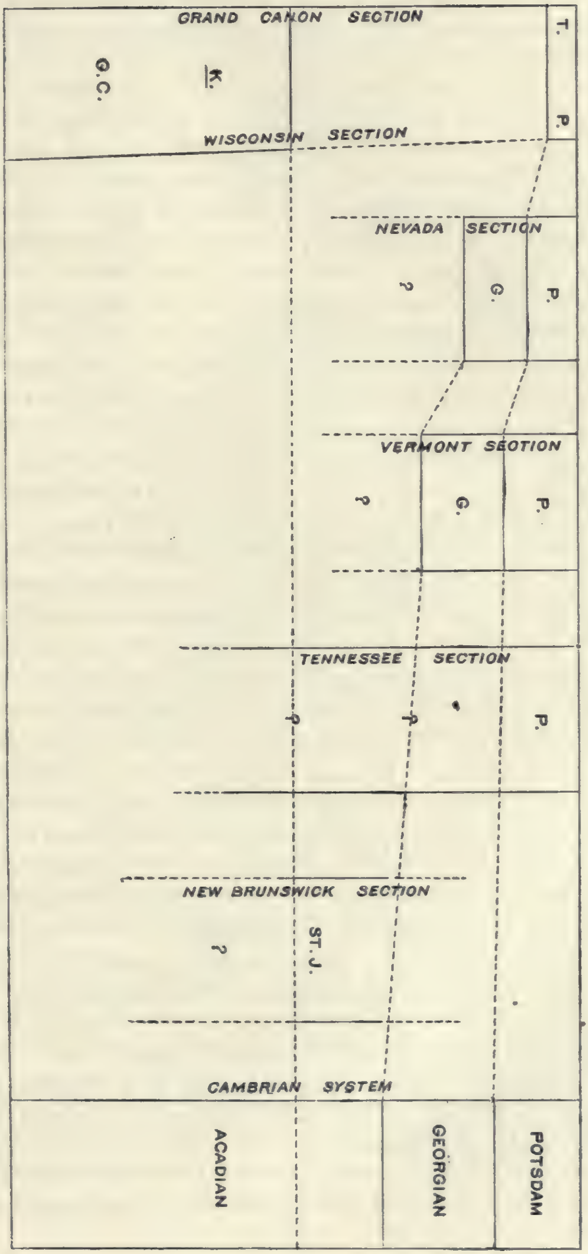
The relations of the Grand Cañon section are shown in the first column of the page of sections.

The Potsdam sandstone in Wisconsin occupies the same relative stratigraphic position as the Tonto formation, except that the break above the Tonto and between it and the Devonian is filled in by the Calciferous and other Silurian formations. As has already been said, the faunas of the Potsdam and Tonto are very much the same in general character. The Potsdam formation here overlies unconformably a series of strata that are directly comparable with the Grand Cañon and Chu-ar′ series. The Keweenawan series, according to Chamberlin, has about 10,000 feet of sedimentary strata distributed through 30,000 feet of eruptive rocks. In all this great mass no decisive evidence of organic life has been discovered, but knowing that the series is unconformably overlain by the Potsdam formation and that it in turn rests unconformably on the Archæan, as does the Grand Cañon series, we feel justified in correlating the Grand Cañon and Wisconsin sections and they are united in the first column of the page of sections.

The upper part of the Nevada section has already been mentioned. Below the Potsdam horizon there occurs a distinct fauna, characterized by a considerable development of the trilobitic genus *Olenellus*, a genus that in the embryonic development of several of its species proves that it is derived from the *Paradoxides* family and is consequently of later date. This section is readily correlated with that of the Georgian group of Vermont, as there we have the Potsdam sandstone above the Olenellus horizon, and in the downward section both stop at nearly the same relative horizon. The position of the Georgian formation in Nevada and Vermont, in relation to the Potsdam, leads to the view that it represents a portion of the period of erosion between the Tonto formation and the Grand Cañon series and also the Potsdam formation and the Keweenawan series.

The upper portion of the Tennessee Cambrian, the Knox shale, is correlated with the Potsdam sandstone, and so is the Knox sandstone. The Chilhowee sandstone and Ocoee conglomerate and slates cannot be directly connected with the Georgian horizon, since the paleontologic data are insufficient. From their position beneath the Knox shale with its Potsdam fauna they are extended downward past the Georgian and into the Paradoxidian or St. Johns horizon. Their total thickness (Geology of Tennessee, pp. 158, 159) is nearly 15,000 feet.

TYPICAL CAMBRIAN SECTIONS.



T. = Tonto. G. C. = Grand Cañon. P. = Potsdam. G. = Georgian. St. J. = St. Johns. K. = Keweenawan.
Maximum thickness of Cambrian = 50,000 + feet.

There is still another group, the St. Johns or Acadian, that occupies an horizon below the Georgian and may fill in a portion of the period of erosion between the pre-Potsdam and Keweenawan and the Tonto and Grand Cañon series, or it may represent some of the upper portions of the Grand Cañon and Keweenawan series. In the geologic sections it is placed beneath the Georgian and as above or passing down into the lower groups. For the present both it and the Paradoxidian argillites of Braintree must be left in doubt with regard to their relations to the lower Cambrian of Wisconsin and northern Arizona.

Of the Canadian survey sections, the one on the north side of the Straits of Belle Isle is most interesting as it gives the Georgian horizon, but unfortunately an interval of ten miles in width is occupied by the straits before the section is again continued. In this interval the Potsdam group is lost, but farther along the coast there occurs, below limestones referred to the Calciferous horizon, a mass of sandstone that may be assigned to the Potsdam formation—giving, in connection with the Olenellus or Georgian horizon, a section not unlike that of Central Nevada.

No other section that has been determined in the British Provinces throws much light on the stratigraphic succession of the Cambrian rocks. At Point Levis a curious mingling of the Cambrian and Silurian faunas has been said to occur, but this is rather to be attributed to error in the interpretation of the stratigraphy in a much disturbed area than to a break in the sequence of organic remains, elsewhere so uniform. I prefer to accept the interpretation given by M. Jules Marcou, who says (The Taconic and Lower Silurian Rocks of Vermont and Canada, Proc. Bos. Soc. Nat. Hist., Vol. VIII, p. 252, 1862,) that the primordial or Cambrian types are associated together and occur in a belt of limestone that contains no traces of the second or Silurian fauna.

The accompanying table of sections gives a general outline of the Cambrian. Numerous local sections of the Potsdam series are not mentioned, as they do not add materially to the general information in regard to the system in its vertical range.

The geographic range is great, extending as it does from Newfoundland to Montana on the northern line, and thence south to Nevada, Texas, and Alabama.

Mr. John Jay Knox made a communication on

THE DISTRIBUTION OF THE SURPLUS MONEY OF THE UNITED STATES AMONG THE STATES.

[Abstract.]

President Jackson, in his message to Congress in 1829, referred to the difficulty in adjusting the tariff, so that the revenues of the Government should be but slightly in excess of its expenditures. He considered the appropriation of money for internal improvements, by Congress, as unconstitutional, but suggested that, if the anticipated surplus in the Treasury should be distributed among the States, according to their ratio of representation, such improvements could then be made by the States themselves. If necessary it would be expedient to propose to the States an amendment to the Constitution, authorizing such legislation.

In his message for the following year he again suggested the same proposition.

The receipts from sales of public lands for the three years, 1834, 1835, and 1836, were $44,492,381—slightly less than the total receipts from this source for the thirty-eight years previous, from 1796 to 1834. On January 1, 1835, the country was virtually out of debt, and the receipts of the Government largely exceeded the previous estimates of the Secretary. The amount of surplus on January 1, 1835, was $8,892,858, and at the same date in 1836 $26,749,803. On January 1, 1837, it amounted to more than forty-two millions.

In 1834–5–6, the public money, which had heretofore been deposited in the Bank of the United States, was deposited in favorite State banks by order of General Jackson. The deposit of the revenues in these banks was followed by financial distress, and during the year 1834, and previous thereto, propositions were made in the public press for distribution of the surplus revenue among the States as a measure of relief. These propositions were first in the form of a distribution of the revenue from public land; then a a distribution of the lands themselves; and finally a distribution of the surplus. During the session of 1835, a select committee was appointed in the Senate, which reported a resolution to amend the Constitution so that the money remaining in the Treasury at the end of each year, until the first of January, 1843, should annually be distributed among the States and Territories. Both General

Jackson and Secretary Woodbury were opposed to this proposition, as the withdrawal of public moneys would deprive the State banks of the deposits, and would be likely to increase the financial troubles. A bill to distribute the surplus was, however, introduced in the Senate, and passed by a vote of 25 to 20. It was evident that this bill could not pass the House, as a majority of its members considered the bill, in the form of a distribution, as unconstitutional. The friends of the measure in the Senate determined to change its form so as to remove the difficulty. A bill then pending in the Senate was so amended as to change the proposition for distribution to a proposition for deposit with the States, and in this form it passed the Senate, and subsequently the House by a large majority, 155 to 38.

This act of June 23, 1836, provided for the deposit with the treasurers of the several States of 37 millions ($37,468,859) in four instalments during the year 1837—the Secretary of the Treasury to receive certificates of deposit therefor signed by competent authority, in such form as he should prescribe, which certificates should express the usual legal obligation, and pledge the faith of the State for the safe keeping and repayment of the deposit, from time to time, whenever the same should be required. The first three installments were deposited. Before the last installment, payable on the 1st day of October, was transferred, a series of financial disasters culminated in the crisis of 1837, and there was no surplus to deposit. Further legislation was deemed necessary in this emergency, and an extra session of Congress was called by President Van Buren. During this session, on September 11, 1837, a bill was reported from the Finance Committee of the Senate, providing that the transfer of the fourth installment should be indefinitely postponed. The opposition to this bill was persistent, and there was a long debate, which was participated in by Webster, Clay, Calhoun, Buchanan, Benton, Silas Wright, Caleb Cushing, and others of the Senate; and in the House by Adams, Fillmore and Sibley of New York, Bell of Tennessee, Wise of Virginia, and many others.

A bill was finally passed, providing for the postponement of the deposit of the fourth installment until January 1, 1839. It passed the House by a vote of 119 to 117, and contained an amendment proposed by Mr. Buchanan, providing that the deposits should not be subject to the requisition of the Secretary of the Treasury, but should remain until called for by Congress. On the 1st of Jan-

uary, 1839, there were no funds in the Treasury available for the payment of the fourth installment, and since that date there has never been a surplus in the Treasury above the debts and estimated expenditures of the Government.

The amount of the three installments was $28,101,645, and the amount placed in the Treasury of each State has since been carried among "unavailable funds of the general Treasury," as may be seen by reference to the annual reports of the Treasurer of the United States.

The fourth installment, amounting to $9,367,215, has never been transferred or deposited, and recently the State of Virginia, through the action of its Legislature, and the State of Arkansas, through the action of its treasurer and one of its United States Senators, has applied to the Secretary of the Treasury for the payment of this last instalment.

It is generally believed that the moneys deposited by the Government with the different States were, for the most part, wasted or employed in works of internal improvement which were unnecessary. The data for a full investigation of this subject are not at hand, but it is known that the States of Massachusetts, Connecticut, New York, New Jersey, Pennsylvania, Delaware, Maryland, North Carolina, Illinois, Indiana, Kentucky, Ohio, and Missouri appropriated a considerable portion of the income from this fund to the support of public schools; and that in many of these States the income from the whole fund has been from the commencement, and still is, devoted to the education of the people.

A bill was introduced by Senator Logan, during the first session of the last Congress, providing that the entire income derived from the internal-revenue tax on the manufacture and sale of distilled spirits shall be appropriated and expended for the education of all children living in the United States, as shown by the census of 1880 and each succeeding census. The bill also provides that the States shall be required, before receiving the benefits of the act, to make school attendance obligatory upon all children between the ages of seven and twelve years, for at least six months in each year.

Mr. Alvord inquired as to the present status of the Smithsonian fund, amounting to about half a million of dollars, which was invested in the bonds of the State of Arkansas.

Mr. Knox said that the Government has assumed the Arkansas

bonds formerly held by the Smithsonian Institution, and that the Government also held quite a large amount of the bonds of the States of Virginia and Arkansas in the Indian Trust Fund. If legislation should be obtained authorizing the payment of the fourth nstalment to these States, such legislation should provide that the payment be made in the bonds now held by the Government.

Mr. ALVORD said that the history of agricultural college grants was not thus far very encouraging. It would have been better if Congress had provided that the agricultural colleges should never be united with other colleges. The union was apt to lead to confusion and controversies, and lower the standard and prestige of both. Witness the case of Dartmouth College. In this reference Mr. MUSSEY concurred.

The Hon. HUGH MCCULLOUGH, being invited by the Chair to participate in the discussion, said that in Indiana the application of the money deposited by the United States had occasioned a long debate, which had resulted in its division. One half, by means of a system of commissioners, was loaned to individuals on land and mortgage; the other half was put into stock of the State Bank, with which the speaker was at that time connected. In a financial crisis the first half was practically lost, probably less than one-twentieth part being recovered; but the loss was fortunately made good by the bank stock, upon which dividends were regularly paid, and by which the investment was eventually doubled. Since the closing of the bank, this money has constituted the school fund of Indiana.

Mr. R. D. CUTTS made a communication on

THE ACTION OF THE INTERNATIONAL GEODETIC ASSOCIATION AS TO AN INITIAL MERIDIAN AND UNIVERSAL TIME.

[Abstract.]

The International Geodetic Association of Europe, formed for the purpose of connecting the systems of triangulation executed by the different States of Europe, and hence for the measurement of arcs, and for the discussion of all questions of science comprised within the term Geodesy, has been in active existence for many years. The meeting in 1882 was held at The Hague, and before adjournment it was decided that the seventh conference should meet at Rome, in October, 1883.

In the meantime, all governments in diplomatic relations with the United States were invited by the President, in accordance with the act of Congress, August 3, 1883, to send delegates to Washington for the purpose of fixing upon a meridian proper to be employed as a common zero of longitude and standard of time, reckoning throughout the globe. More than twenty of these countries had signified, before October last, their acceptance of the invitation, but these did not include many of the principal governments of Europe. The delay in forwarding their definitive replies was due to their desire to have the advice, before committing themselves, of the Eurpean Geodetic Association. Hence it was at the request of many of these governments that the Association took up the subject of the unification of longitudes, and of the introduction of a universal time.

So soon as it was decided to take such action, General Ibanez, of Spain, the then President of the Association, addressed a letter to the Superintendent of the Coast and Geodetic Survey, urging him in strong terms to send a delegate to the meeting at Rome. So short a notice was given, however, that the delegate selected had to start at once, reaching Rome only on the morning of the first day's session, October 15th.

After a full discussion of the different views presented, the following resolutions were almost unanimously passed on October 24th. It must be borne in mind that they are merely of an advisory character, sanctioned and urged, nevertheless, by the highest scientific authority. It is the function of the convention to be held at Washington next year to take official and decisive action on the subject in all its details.

Resolutions of the International Geodetic Commission in relation to the Unification of Longitudes and of Time.

The seventh general conference of the International Geodetic Association, held at Rome, and at which representatives of Great Britain, together with the directors of the principal astronomical and nautical almanacs, and a delegate from the Coast and Geodetic Survey of the United States, have taken part, after having deliberated upon the unification of longitude by the adoption of a single initial meridian, and upon the unification of time by the adoption of a universal hour, have agreed upon the following resolutions:

I. The unification of longitude and of time is desirable, as much in the interest of science as in that of navigation, of commerce, and of international communication. The scientific and practical utility of this reform far outweighs the sacrifice of labor and the difficulties of adaptation which it would entail. It should, therefore, be recommended to the Governments of all the States interested, to be organized and confirmed by an International Convention, to the end that hereafter one and the same system of longitudes shall be employed in all the institutes and geodetic bureaus, for the general geographic and hydrographic charts, as well as in the astronomical and nautical almanacs, with the exception of those made to preserve a local meridian, as, for instance, the almanacs for transits, or those which are needed to indicate the local time, such as the establishment of the port, &c.

II. Notwithstanding the great advantages which the general introduction of the decimal division of a quarter of the circle in the expressions of the geographical and geodetic co-ordinates, and in the corresponding time expressions, is destined to realize for the sciences and their applications, it is proper, through considerations eminently practical, to pass it by in considering the great measure of unification proposed in the first resolution.

However, with a view to satisfying, at the same time, very serious scientific considerations, the Conference recommends, on this occasion, the extension by the multiplication and perfection of the necessary tables, of the application of the decimal division of the quadrant, at least, for the great operations of numerical calculations, for which it presents incontestable advantages, even if it is wished to preserve the old sexagesimal division for observations, for charts, navigation, &c.

III. The Conference proposes to the Governments to select for the initial meridian that of Greenwich, defined by a point midway between the two pillars of the meridian instrument of the Observatory of Greenwich, for the reason that that meridian fulfils, as a point of departure for longitudes, all the conditions demanded by science; and because being at present the best known of all, it presents the greatest probability of being generally accepted.

IV. It is advisable to count all longitudes, starting from the meridian of Greenwich, in the direction from west to east only.

V. The Conference recognizes for certain scientific wants and for the internal service in the chief administrations of routes of com-

munication, such as the railroads, steamship lines, telegraphic and post routes, the utility of adopting a universal time, along with local or national time, which will necessarily continue to be employed in civil life.

VI. The Conference recommends, as the point of departure of universal time and of cosmopolitan date, the mean noon of Greenwich which coincides with the instant of midnight, or with the commencement of the civil day, under the meridian situated 12 hours or 180 degrees from Greenwich.

It is agreed to count the universal time from 0^h to 24^h.

VII. It is desirable that the States which, for the purpose of adopting the unification of longitudes and of time, find it necessary to change their meridians, should introduce the new system of longitudes and of hours as soon as possible.

It is equally advisable that the new system should be introduced without delay in teaching.

VIII. The Conference hopes that if the entire world should agree upon the unification of longitudes and of time by accepting the meridian of Greenwich as the point of departure, Great Britian will find in this fact an additional motive to make, on its part, a new step in favor of the unification of weights and measures, by acceding to the *Convention du Mètre* of the 20th May, 1875.

IX. These resolutions will be brought to the knowledge of the Governments and recommended to their favorable consideration, with the expression of a hope that an International Convention, confirming the unification of longitudes and of time, shall be concluded as soon as possible, by means of a special conference, such as the Government of the United States has proposed.

Mr. HILGARD said that while the report of the Association did not conform in some of its details to the desires and interests of this country, nevertheless our principal object had been gained by the endorsement of the Association for the International Conference on the subject of standard time, to be held in Washington.

The selection of the meridian of Greenwich as the starting point for longitudes, was more convenient for us than for Europeans; Europeans alone are liable to the confusion arising from the numerical identity of meridians east and west of Greenwich. It will be impossible, however, for us to agree to the rule which counts all longitudes from west to east.

Mr. ELLIOTT opposed the establishment of noon as the initial hour of the day. It seemed to be proposed in the interest of astronomers, who work at night, and would not be submitted to by the people at large.

He exhibited a map showing a grouping of the railroads of the country under the recently adopted time schedule.

Mr. CUTTS said that the resolutions of the Geodetic Association do not appertain to civil time. The "universal time" they advocate is for the use only of astronomers and great transportation corporations.

Other remarks were made by Mr. NEWCOMB.

242D MEETING. DECEMBER 8, 1883.

By permission of the Secretary of the Smithsonian Institution, the Society occupied for the evening the Lecture Hall of the National Museum.

The President called Vice-President MALLERY to the Chair.

There were present about three hundred members and guests.

By invitation, the Presidents of the Biological and Anthropological Societies occupied seats on the platform.

The President of the Society, Mr. J. W. POWELL, delivered the annual address, taking for his subject

THE THREE METHODS OF EVOLUTION.

[The address is printed on pages XXVII–LII, *ante*.]

The Chair invited the members of the Society and their friends to remain for a period after adjournment, for the purpose of social intercourse.

The Society then adjourned.

243D MEETING. DECEMBER 22, 1883.

THE THIRTEENTH ANNUAL MEETING.

The President in the chair.

Thirty-four members present.

The minutes of the 226th, 241st, and 242d meetings were read.

The Chair announced the death, since the last meeting, of General R. D. CUTTS.

The Chair announced the election to membership of Messrs. ROBERT SIMPSON WOODWARD, DANIEL ELMER SALMON, and JOHN MILLS BROWNE.

The Secretary's report on the membership of the Society was read. During the year the Society received seventeen new members, lost eight by death, and lost three by resignation.

The Treasurer not being present, the Chair appointed Mr. Henry Farquhar Treasurer *pro tempore.*

The officers for the ensuing year were then elected by ballot. (The list is printed on page xv.)

On motion of Mr. JENKINS, the vote for President was made unanimous.

The Chair appointed Messrs. C. A. White, S. Newcomb, and H. C. Yarrow a committee to audit the annual report of the Treasurer.

The Society then adjourned.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

MATHEMATICAL SECTION.

STANDING RULES

OF THE

MATHEMATICAL SECTION.

Adopted March 24, 1883.

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1. The object of this Section is the consideration and discussion of papers relating to pure or applied mathematics.

2. The special officers of the Section shall be a Chairman and a Secretary, who shall be elected at the first meeting of the Section in each year, and discharge the duties usually attaching to those offices.

3. To bring a paper regularly before the Section it must be submitted to the Standing Committee on Communications for the stated meetings of the Society, with the statement that it is for the Mathematical Section.

4. Meetings shall be called by the Standing Committee on Communications whenever the extent or importance of the papers submitted and approved appear to justify it.

5. All members of the Philosophical Society who wish to do so may take part in the meetings of this Section.

6. To every member who shall have notified the Secretary of the General Committee of his desire to receive them, announcements of the meetings of the Section shall be sent by mail.

7. The Section shall have power to adopt such rules of procedure as it may find expedient.

LIST OF MEMBERS

WHO RECEIVE ANNOUNCEMENT OF MEETINGS OF THE

MATHEMATICAL SECTION.

Abbe, C.
Alvord, B.
Avery, R. S.
Babcock, O. E.
Baker, M.
Bates, H. H.
Billings, J. S.
Burgess, E. S.
Christie, A. S.
Coffin, J. H. C.
DeLand, T. L.
Doolittle, M. H.
Eastman, J. R.
Elliott, E. B.
Farquhar, H.
Flint, A. S.
Gilbert, G. K.
Newcomb, S.
Gore, J. H.
Green, B. R.
Hall, A.
Harkness, W.
Hazen, H. A.
Hilgard, J. E.
Hill, G. W.
King, A. F. A.
Kummell, C. H.
Lefavour, E. B.
Peirce, C. S.
Ritter, W. F. M'K.
Smiley, C. W.
Taylor, W. B.
Upton, W. W.
Walling, H. F.
Winlock, W. C.

INAUGURAL ADDRESS

OF THE

CHAIRMAN OF THE MATHEMATICAL SECTION,

BY ASAPH HALL.

GENTLEMEN OF THE MATHEMATICAL SECTION:

I thank you for the honor you have conferred on me by my election as Chairman of this Section, and the best return that I can make is to do my utmost to render our meetings as interesting and successful as possible.

Although my duties have been such that I have not been able to take a very active part in the proceedings of the Philosophical Society, it is easy to understand how a need has been felt for a more full and frequent discussion of mathematical questions. Mathematics has indeed been called the queen of the sciences, but the rigor and dryness of its methods make it distasteful to many. The fact seems to be that as any branch of knowledge advances and finally is reduced to law, it loses in a large degree its attractiveness and popularity. Then, it is only with the indefinite outlines and the obscure boundaries of this science that most people like to deal; and this may be natural and right, since nearly all advancement originates in speculation and doubt, which lead to investigation, and which, by a variety of motives, spur men on to labor. But the science of mathematics, though old, is yet young and vigorous. We have now six journals of the highest rank, which are devoted almost exclusively to pure mathematics—two in Germany, two in France, one in England, and, I am glad to say, one in our own country. These journals are devoted to the discussion of the highest conceptions of space and number, treating chiefly of the laws and forms of analytical expressions, and generally they touch lightly on any practical application of the science. Such discussions prepare the way, however, for better and more general practical methods, and in our own country they have, I think, another value. For one, I can hardly accept the doctrine, advocated in some quarters, that the American scientific man of the future should

be distinguished by his facility in getting a patent on his discovery, in forming joint stock companies and watering stock, and in suddenly becoming rich at the expense of his fellow-men. Such a career may be a natural result of our present system of sociology, but it does not seem to be in harmony with scientific thought and research, and our social need is for men of a different character. Far nobler is the life of one who devotes himself to the study of the most abstract forms of science; winning for us, if haply he may, another forward step up the hill of knowledge.

But when we come to the field of applied mathematics we soon learn how necessary are the studies of the pure mathematician. Nearly all the researches in natural philosophy, where the action of forces is concerned, require the formation and solution of differential equations, and hence the theory of such equations becomes important, and in some cases almost essential, for the advancement of physical investigations. It is not, of course, to be supposed that experiment and observation are to be done away with or neglected, or that mere skill in differentiating, integrating, and solving equations can supply the place of correct thinking. In fact, we may be sure that Leibnitz was mistaken when he declared that the invention of the differential calculus had made known that royal road to knowledge for which the king had inquired in vain of Euclid. But still it remains true that this calculus forms the most powerful engine we have for the solution of questions in natural philosophy. It enables us to adopt the old maxim, "*divide et impera.*" If we can reduce the problem to its elements, and can form its true differential equation, the rest of the work is purely mathematical. Unfortunately, the differential equations that occur in the problems of nature are very different from those given in our text-books, and their exact solution is in most cases impossible. Here we must rely chiefly on that happy device of the variation of constants, by means of which the solution of simpler forms is extended to the more complex.

One of the great advantages of putting a question in a mathematical form is the precision with which it can be stated. If we are right, the truth of our assertion will be the sooner acknowledged, and if we are wrong, our error can be the more easily detected. Frequently it has seemed to me that disputes would be avoided in the meetings of our scientific societies if men would take the trouble to put their assertion into a formula and write it on the blackboard;

and certainly there would be a clearness and meaning that are so often wanting. Thus, if any one asserts that when a planet comes to its perihelion it ought to fall into the sun, the law of gravitation being true, he is not worth listening to unless he will put his assertion into a formula; and when he is able to do this he will probably find out his own error. There will be so much gain by simply reducing the problem to its elements and giving it a correct form. Again, where scientific statements *may be* true, there will be a gain in giving them, when possible, a mathematical expression. Thus, when we are told that the fixed star 1830 Groombridge is running away, disobedient to the law of gravitation, how much better it would be if we could see on the blackboard the mathematical proof of this assertion, so that we could judge for ourselves on what assumption it is based. The subject of impulsive forces is one that we hear disputes about in our own society, and it seems to be a fair field for a mathematical exposition. How often do we see such phrases as "energy," "potential energy," "kinetic energy," "conservation of energy," "work," "virial," &c. Could not some one of our members give us a clear account of these terms, show us how they are connected with the general equations of mechanics, what new ideas they contain, and on what limitations they may be based? As the application of mathematics is extended, sounding phrases are sure to come into use, and it is well to test them and know what they mean.

In the discussions of this Section, while all are invited to be critical, I trust that we shall all be kind and good tempered. We come together for discussion and mutual improvement, and while error is not to be spared we must be charitable to each other's faults.

BULLETIN

OF THE

MATHEMATICAL SECTION.

A communication signed by Mr. J. E. Hilgard and nineteen other members of the Philosophical Society, asking that a Section in Mathematical Science be formed, as provided in Paragraph 6 of the Standing Rules of the Society, was presented to the General Committee at its regular meeting January 27, 1883. The proposition was agreed to, and Mr. Hilgard was empowered to call a special meeting for the purpose of organizing such a section; the call being extended to all members of the Society.

1st Preliminary Meeting. February 17, 1883.

Twelve members met in the library of the Army Medical Museum, in answer to the first call.

Mr. Hilgard not being present, Mr. E. B. Elliott was called to the Chair.

An informal discussion followed, which brought out a unanimous sentiment in favor of forming the Section.

With some differences of opinion as to details, it was agreed to postpone formal action, and the meeting adjourned subject to call.

2d Preliminary Meeting. March 5, 1883.

Mr. Hilgard in the Chair.

Fifteen members present.

A plan of organization was adopted, and referred to the General Committee of the Society for consideration.

1st Regular Meeting. March 29, 1883.

Fourteen members present.

In the absence of Mr. Hilgard, who had presided over the meeting for organization, Mr. G. W. Hill was called to the Chair.

The standing rules for the government of the Section, as adopted at the last meeting of the General Committee of the Society, were read.

The Section then proceeded to elect officers for the year 1883. On motion of Mr. Winlock the rules of the Society at its Annual Meeting were followed.

Mr. Asaph Hall was chosen Chairman and Mr. H. Farquhar Secretary.

A letter from Mr. Marcus Baker, dated Los Angeles, Cal., was read by Mr. Christie. It expressed a strong interest in the Section, recommending that it should be conducted as nearly as possible on the plan devised by the late Prof. Henry for the Society itself, by which business and science are kept apart. A free use of pencil and paper at the meetings, and seats around a table, were further suggested. The letter closed by advocating the foundation of a new mathematical journal.

Mr. Christie then made a communication on

A QUASI GENERAL DIFFERENTIATION.

The paper was discussed by Messrs. Kummell, Elliott, Hill, and Doolittle. The author reserves it from publication to await further research.

A resolution was passed, requesting the committee in charge of the matter to call meetings of the Section on Wednesday evenings.

2d Meeting. April 11, 1883.

The Chairman, Mr. Hall, presided.

Present, ten members and two invited guests.

It was announced that the Editor of "Science" would publish brief reports of the meetings of the Section.

The Chairman read an inaugural address, [given in full on pp. 117 to 119 *ante.*]

Mr. C. H. Kummell then began a paper on

ALIGNMENT CURVES,

which was not finished at the time of adjournment.

3d Meeting. April 26, 1883.

The Chairman presided.

Present, sixteen members and one invited guest.

Mr. Kummell completed his paper, begun at the second meeting, on

ALIGNMENT CURVES ON ANY SURFACE, WITH SPECIAL APPLICATION TO THE ELLIPSOID.

[Abstract.]

The attempt to put a number of points in line on a curved surface whose normals are supposed to be given (abstraction is made of deviations of the plumb-line and lateral refraction) gives rise to various curves, which I call alignment curves. There are two classes—alignment curves with two given termini and those with a starting point only. There are three distinct curves of the first class, viz.: 1. The normal section, if the surveyor directs his assistant to place staffs in line from one end of the line. 2. A curve described if the surveyor would align a point near him, then move up to this point, thence align another point, etc., until the terminus is reached. This process is that used in chaining, or more roughly by a pedestrian going towards a point, and is characterized by requiring only foresights. I call it *proörthode* (πρὸ, ὀρθὸς, ὁδός).* 3. A curve resulting if a backsight is also taken. This curve is therefore defined by the condition that the normal plane at any point of it which passes through one end also passes through the other. I call it *diorthode* (διὰ, ὀρθὸς, ὁδός), because it may be con-

* This and other names of curves were coined by my friend, Mr. Wm. R. Galt, of Norfolk, Va.

sidered straight *all through* at any of its points. This curve may be considered the ideal curve of a primary base line. Various names have been given to it when on the terrestrial spheroid. Dr. Bremiker, who appears to have first considered it (in his Studien ueber hoehere Geodæsie, 1869), proposed the name "Feldlinie"; that is, field line. He thinks it should be adopted as the geodetic line, because both linear and angular measurements conform to it. Clarke, Zachariæ, and Helmert have also mentioned it, the latter, however, only in a note, where he remarks that it deserves no consideration in geodesy.

To the second class belong two curves: 1. A curve described as follows: The surveyor at the starting point takes his directions from a staff at short distance and directs his assistant to place a staff in the prolongation. Repeating this operation from the first staff, from the second staff, etc., he describes a curve which is well known to be the shortest curve between any of its points. It is usually called the geodetic line. However, since this name would apply at least equally well to the three curves already considered, I propose the name *brachisthode* (*βράχιστος*). The properties of this curve need not be considered here, such mathematicians as Gauss, Hansen, Bessel, and others, having perfected its theory. Helmert, in his "Hoehere Geodæsie," makes this curve the basis of nearly all geodetic computations. The brachisthodic process on a plane evidently results in a straight line, and on a sphere in a great circle. If, on these surfaces, it is in starting directed to a distant point, that point will be reached (disregarding errors of observation). Not so on other curved surfaces; there, in general, the first element of the brachisthode is not in direction to any of its points at a finite distance. 2. The *loxodrome* being a curve which has a constant inclination to a given direction, may, perhaps, be mentioned as belonging to this class.

The general equations of the two-end curves on any surface may be developed as follows:

Let the equation of the surface be:

$$u = f(x, y, z) = 0 \qquad (1)$$

then if (ξ, η, ζ) is any point in the normal at the surface point (x, y, z), we have its equations:

$$\frac{\xi - x}{\left(\frac{du}{dx}\right)} = \frac{\eta - y}{\left(\frac{du}{dy}\right)} = \frac{\zeta - z}{\left(\frac{du}{dz}\right)} \qquad (2)$$

and the equation of a normal plane at the surface point (x, y, z) and passing through (x_2, y_2, z_2), (not necessarily a surface point, but considered so here), is:

$$0=\left[(\xi-x)\left(\frac{du}{dz}\right)-(\zeta-z)\left(\frac{du}{dx}\right)\right]\left[(y_2-y)\left(\frac{du}{dz}\right)-(z_2-z)\left(\frac{du}{dy}\right)\right]$$

$$-\left[(\eta-y)\left(\frac{du}{dz}\right)-(\zeta-z)\left(\frac{du}{dy}\right)\right]\left[(x_2-x)\left(\frac{du}{dz}\right)-(z_2-z)\left(\frac{du}{dx}\right)\right]$$

$$=[(y_2-y)(\xi-x)-(x_2-x)(\eta-y)]\left(\frac{du}{dz}\right)$$

$$+[(z_2-z)(\eta-y)-(y_2-y)(\zeta-z)]\left(\frac{du}{dx}\right)$$

$$+[(x_2-x)(\zeta-z)-(z_2-z)(\xi-x)]\left(\frac{du}{dy}\right) \tag{3}$$

If in this we replace the surface point (x, y, z) by the surface point (x_1, y_1, z_1) and (ξ, η, ζ) by the surface point (x, y, z) we obtain:

$$0=[(y_2-y_1)(x-x_1)-(x_2-x_1)(y-y_1)]\left(\frac{du}{dz_1}\right)$$

$$+[(z_2-z_1)(y-y_1)-(y_2-y_1)(z-z_1)]\left(\frac{du}{dx_1}\right)$$

$$+[(x_2-x_1)(z-z_1)-(z_2-z_1)(x-x_1)]\left(\frac{du}{dy_1}\right) \tag{4}$$

which, if combined with the equation of the surface, gives the normal section at (x_1, y_1, z_1) through (x_2, y_2, z_2).

If, however, we replace in (3) (ξ, η, ζ) by the surface point (x, y, z) we obtain:

$$0=[(y_2-y)(x_1-x)-(x_2-x)(y_1-y)]\left(\frac{du}{dz}\right)$$

$$+[(z_2-z)(y_1-y)-(y_2-y)(z_1-z)]\left(\frac{du}{dx}\right)$$

$$+[(x_2-x)(z_1-z)-(z_2-z)(x_1-x)]\left(\frac{du}{dy}\right) \tag{5}$$

and this, combined with the equation of the surface, gives the diorthodic curve.

As we move along the diorthode, (5) may be considered a plane which turns about the chord (1, 2) as an axis, so as to be always normal to the surface. It follows that the normals at any point of the diorthode are constrained to pass through the chord. They will thus generate a ruled surface, whose equation is not (5) however.

The equation of this ruled surface is obtained by eliminating x, y, z from (1), (2), and (5). It is important to remark that the diorthode does not consist of parts which are diorthodes with respect to their termini, otherwise the normals would at the same time pass through two chords from the same point and the curve would be a plane curve. Dr. Bremiker had erroneously supposed that the diorthode was touched by the normal planes. This is only the case at the termini. He has been criticized by Dr. Bruns of Pulkowa and by Helmert, but neither critic has shown the existence of a curve possessing this property, namely, the proörthode, in which the normal plane at any of its points passes through the consecutive point and the forward terminus, but not in general through the starting point. If then in (5) we replace (x_1, y_1, z_1) by $(x + dx, y + dy, z + dz)$ we have:

$$
\begin{aligned}
0 &= [(y_2 - y)\,dx - (x_2 - x)\,dy]\left(\frac{du}{dz}\right) \\
&\quad + [(z_2 - z)\,dy - (y_2 - y)\,dz]\left(\frac{du}{dx}\right) \\
&\quad + [(x_2 - x)\,dz - (z_2 - z)\,dx]\left(\frac{du}{dy}\right) \\
&= \left[(y_2 - y)\left(\frac{du}{dz}\right) - (z_2 - z)\left(\frac{du}{dy}\right)\right] dx \\
&\quad + \left[(z_2 - z)\left(\frac{du}{dx}\right) - (x_2 - x)\left(\frac{du}{dz}\right)\right] dy \\
&\quad + \left[(x_2 - x)\left(\frac{du}{dy}\right) - (y_2 - y)\left(\frac{du}{dz}\right)\right] dz \qquad (6)
\end{aligned}
$$

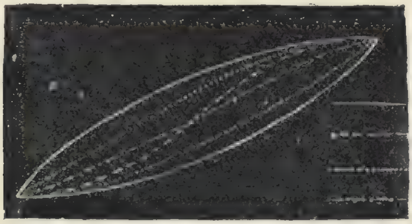
By means of the equation of the surface (1) and its differential equation

$$0 = \left(\frac{du}{dx}\right) dx + \left(\frac{du}{dy}\right) dy + \left(\frac{du}{dz}\right) dz \qquad (7)$$

any one of the variables with its differential can be eliminated. The resulting differential equation being integrated so as to contain the starting point (x_1, y_1, z_1), will be the equation of a projection of the proörthode on a coördinate plane.

The proörthode being differently related to its ends, will be different forward and backward, while the diorthode is the same forward and backward.

The following diagram will illustrate the relative course of these curves:



Any surface of the second degree may be represented by

$$u = 0 = \left(\frac{x-a}{a}\right)^2 + \frac{y^2}{p} + \frac{z^2}{q} - a \qquad (8)$$

The origin is taken at one of its real vertices, so that $(a, 0, 0)$ is its centre. The equation of the diorthode is then by (5), if we write $x_2 - x_1 = \Delta x$; $y_2 - y_1 = \Delta y$; $z_2 - z_1 = \Delta z$,

$$0 = [(y_2 - y)(x_1 - x) - (x_2 - x)(y_1 - y)] \frac{z}{q}$$

$$+ [(z_2 - z)(y_1 - y) - (y_2 - y)(z_1 - z)] \frac{x-a}{a}$$

$$+ [(x_2 - x)(z_1 - z) - (z_2 - z)(x_1 - x)] \frac{y}{p}$$

$$= (y_2 x_1 - y_1 x_2 + y\Delta x - x\Delta y) \frac{z}{q}$$

$$+ (z_2 y_1 - z_1 y_2 + z\Delta y - y\Delta z) \frac{x-a}{a}$$

$$+ (x_2 z_1 - x_1 z_2 + x\Delta z - z\Delta x) \frac{y}{p}$$

$$= \Delta x \left(\frac{1}{q} - \frac{1}{p}\right) yz + \Delta y \left(\frac{1}{a} - \frac{1}{q}\right) xz + \Delta z \left(\frac{1}{p} - \frac{1}{a}\right) xy$$

$$+ (z_2 y_1 - z_1 y_2) \frac{x-a}{a}$$

$$+ (x_2 z_1 - x_1 z_2 + p\Delta z) \frac{y}{p} + (y_2 x_1 - y_1 x_2 - q\Delta y) \frac{z}{q} \qquad (9)$$

The equations of the chord (1, 2) may be written:

$$\frac{x_2 - x}{x_1 - x} = \frac{y_2 - y}{y_1 - y} = \frac{z_2 - z}{z_1 - z} \qquad (10)$$

Every point of the chord, therefore, satisfies (9), and since that

represents a surface of the second degree, it must be a hyperboloid of one sheet, for this and its varieties are the only ruled surfaces of that order. In the general form (9) it has a center in finite space. It is then the elliptic hyperboloid; but if $a = p$ (or $a = q$ or $p = q$), it has its center at an infinite distance, and it is a parabolic hyperboloid. In this case the base surface becomes:

$$0 = \frac{(x - a)^2 + y^2}{a} + \frac{z^2}{q} - a \tag{11}$$

which is a surface of revolution of the second degree.

If $a = p = q$, then (9) becomes a plane and the base surface a sphere. (9) is evidently satisfied by the center $(a, 0, 0)$, therefore the intersecting surface always passes through the center of the base surface.

I consider now the ellipsoid:

$$0 = \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} - 1 \tag{12}$$

We have then the intersecting surface of the diorthode:

$$0 = (z_2 y_1 - z_1 y_2 + z\Delta y - y\Delta z)\frac{x}{a^2} + (x_2 z_1 - x_1 z_2 + x\Delta z - z\Delta x)\frac{y}{b^2}$$

$$+ (y_2 x_1 - y_1 x_2 + y\Delta x - x\Delta y)\frac{z}{c^2} \tag{13}$$

Let $(0, y_x, z_x)$ be the point where the chord (1, 2) pierces the yz - plane
$(x_y, 0, z_y)$ " " " " " zx - "
$(x_z, y_z, 0)$ " " " " " xy - "

then we can easily verify the relations:

$$y_x = \frac{x_2 y_1 - x_1 y_2}{\Delta x} \; ; \; z_x = \frac{x_2 z_1 - x_1 z_2}{\Delta x} \tag{14$_x$}$$

$$z_y = \frac{y_2 z_1 - y_1 z_2}{\Delta y} \; ; \; x_y = \frac{y_2 x_1 - y_1 x_2}{\Delta y} \tag{14$_y$}$$

$$x_z = \frac{z_2 x_1 - z_1 x_2}{\Delta z} \; ; \; y_z = \frac{z_2 y_1 - z_1 y_2}{\Delta z} \tag{14$_z$}$$

and if we assume:

$$\alpha_b^2 = 1 - \frac{a^2}{b^2} \; ; \; \alpha_c^2 = 1 - \frac{a^2}{c^2} \tag{15$_a$}$$

$$\beta_c^2 = 1 - \frac{b^2}{c^2} \; ; \; \beta_a^2 = 1 - \frac{b^2}{a^2} \tag{15$_b$}$$

$$\gamma_a^2 = 1 - \frac{c^2}{a^2} \; ; \; \gamma_b^2 = 1 - \frac{c^2}{b^2} \tag{15$_c$}$$

(13) will take either of the following equivalent forms:

$$0 = \Delta z\,(y_z - \alpha_b^2 y)\,\frac{x}{a^2} + \Delta x\,(z_x - \beta_c^2 z)\frac{y}{b^2} + \Delta y\,(x_y - \gamma_a^2 x)\frac{z}{c^2} \quad (13^{\text{I}})$$

$$0 = \Delta y\,(z_y - \alpha_c^2 z)\,\frac{x}{a^2} + \Delta z\,(x_z - \beta_a^2 x)\frac{y}{b^2} + \Delta x\,(y_x - \gamma_b^2 y)\frac{z}{c^2} \quad (13^{\text{II}})$$

The following relations will be much referred to:

$$0 = \frac{y_z}{\Delta y} + \frac{z_y}{\Delta z} = \frac{z_x}{\Delta z} + \frac{x_z}{\Delta x} = \frac{x_y}{\Delta x} + \frac{y_x}{\Delta y} \qquad (16)$$

$$\frac{x_y - x_z}{x_y} = \frac{y_z}{y_x} = \frac{z_y}{z_y - z_x}\,; \quad \frac{y_z - y_x}{y_z} = \frac{z_x}{z_z} = \frac{x_z}{x_z - x_y}\,;$$

$$\frac{z_x - z_y}{z_x} = \frac{x_y}{x_z} = \frac{y_x}{y_x - y_z} \qquad (17)$$

$$0 = x_y\, y_z\, z_x + y_x\, z_y\, x_z \qquad (18)$$

Replacing in these Δx, Δy, Δz; y_x, z_y, x_z; z_x, x_y, y_z

$$\text{by } \frac{1}{a^2}, \frac{1}{b^2}, \frac{1}{c^2}\,;\ \alpha_b^2, \beta_c^2, \gamma_a^2\,;\ \alpha_c^2, \beta_a^2, \gamma_b^2 \qquad (19)$$

$$\text{we have: } 0 = \frac{\gamma_b^2}{c^2} + \frac{\beta_c^2}{b^2} = \frac{\alpha_c^2}{a^2} + \frac{\gamma_a^2}{c^2} = \frac{\beta_a^2}{b^2} + \frac{\alpha_b^2}{a^2} \qquad (16^{\text{I}})$$

$$\frac{\beta_a^2 - \gamma_a^2}{\beta_a^2} = \frac{\gamma_b^2}{\alpha_b^2} = \frac{\beta_c^2}{\beta_c^2 - \alpha_c^2}\,; \quad \frac{\gamma_b^2 - \alpha_b^2}{\gamma_b^2} = \frac{\alpha_c^2}{\beta_c^2} = \frac{\gamma_a^2}{\gamma_a^2 - \beta_a^2}\,;$$

$$\frac{\alpha_c^2 - \beta_c^2}{\alpha_c^2} = \frac{\beta_a^2}{\gamma_a^2} = \frac{\alpha_b^2}{\alpha_b^2 - \gamma_b^2} \qquad (17^{\text{I}})$$

$$0 = \beta_a^2\gamma_b^2\alpha_c^2 + \alpha_b^2\beta_c^2\gamma_a^2 \qquad (18^{\text{I}})$$

and these relations also will be found correct.

Because in the equation of the diorthodic surface the terms in x^2, y^2, z^2 are wanting, there must be lines, perpendicular to the coordinate planes, lying wholly in the surface. To determine those perpendicular to the xy-plane, I place $= 0$ the term in (13^{I}) dependent on z and that in (13^{II}) independent of z, or

$$0 = -\,y\Delta x\,\frac{\beta_c^2}{b^2} + \frac{\Delta y}{c^2}(x_y - \gamma_a^2 x)$$

$$= -\,\frac{\gamma_b^2}{\gamma_a^2}\cdot\frac{x_y}{y_x}\,y + \left(\frac{x_y}{\gamma_a^2} - x\right) \text{ by (16) and } (16^{\text{I}})$$

$$0 = \Delta y z_y\,\frac{x}{a^2} + \Delta z\,(x_z - \beta_a^2 x)\frac{y}{b^2}$$

$$= \frac{y_z}{\alpha_b^2}\,x + \left(\frac{x_z}{\beta_a^2} - x\right)y \text{ by (16) and } (16^{\text{I}})$$

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Substituting the value of y from the first into the second equation we have:

$$0 = \frac{y_z}{\alpha_b^2}x + \frac{\gamma_a^2}{\gamma_b^2}\cdot\frac{y_x}{x_y}\left(\frac{x_y}{\gamma_a^2} - x\right)\left(\frac{x_z}{\beta_a^2} - x\right)$$

$$= \frac{\gamma_b^2}{\alpha_b^2\gamma_a^2}\cdot\frac{y_z\,x_y}{y_x}\,x + \left(\frac{x_y}{\gamma_a^2} - x\right)\left(\frac{x_z}{\beta_a^2} - x\right);\text{ or,}$$

$$0 = \frac{\beta_a^2 - \gamma_a^2}{\beta_a^2\gamma_a^2}(x_y - x_z)\,x + \left(\frac{x_y}{\gamma_a^2} - x\right)\left(\frac{x_z}{\beta_a^2} - x\right)\text{by } (17_1) \text{ and } (17_1^1)$$

$$= x^2 - \left(\frac{x_y}{\beta_a^2} + \frac{x_z}{\gamma_a^2}\right)x + \frac{x_y}{\beta_b^2}\cdot\frac{x_z}{\gamma_a^2}\therefore x = \frac{x_z}{\gamma_a^2} \text{ or } x = \frac{x_y}{\beta_a^2}$$

Corresponding to the first value we have:

$$y = \frac{\gamma_a^2}{\gamma_b^2}\cdot\frac{y_x}{x_y}\cdot\frac{x_y - x_z}{\gamma_a^2} = \frac{y_x}{\gamma_b^2}\cdot\frac{x_y - x_z}{x_y} = \frac{y_z}{\gamma_b^2} \text{ by } (17_1)$$

and corresponding to the second:

$$y = \frac{\gamma_a^2}{\gamma_b^2}\cdot\frac{y_x}{x_y}\cdot x_y\left(\frac{1}{\gamma_a^2} - \frac{1}{\beta_a^2}\right) = \frac{y_x}{\gamma_b^2}\cdot\frac{\beta_a^2 - \gamma_a^2}{\beta_a^2} = \frac{y_x}{\alpha_b^2} \text{ by } (17_1^1)$$

Denoting these constants by x_c, x_b, y_c, y_a, respectively, we have then the equations of a pair of generatrices of the hyperboloid (13) perpendicular to the xy - plane:

$$x = \frac{x_z}{\gamma_a^2} = x_c\,;\; y = \frac{y_z}{\gamma_b^2} = y_c \tag{20_z}$$

$$x = \frac{x_y}{\beta_a^2} = x_b\,;\; y = \frac{y_x}{\alpha_b^2} = y_a \tag{20_z^1}$$

Similarly the pair of generatrices perpendicular to the yz - plane:

$$y = \frac{y_x}{\alpha_b^2} = y_a\,;\; z = \frac{z_x}{\alpha_c^2} = z_a \tag{20_x}$$

$$y = \frac{y_z}{\gamma_b^2} = y_c\,;\; z = \frac{z_y}{\beta_c^2} = z_b \tag{20_x^1}$$

and that perpendicular to the zx - plane:

$$z = \frac{z_y}{\beta_c^2} = z_b\,;\; x = \frac{x_y}{\beta_a^2} = x_b \tag{20_y}$$

$$z = \frac{z_x}{\alpha_a^2} = z_a\,;\; x = \frac{x_z}{\gamma_a^2} = x_c \tag{20_y^1}$$

Now the second line of each pair intersects the chord, as may be proved thus: The equations of the chord (1, 2) are any two of the following three equations:

$$\frac{x}{x_y} + \frac{y}{y_x} - 1 = 0 \tag{21_z}$$

$$\frac{y}{y_z} + \frac{z}{z_y} - 1 = 0 \tag{21_x}$$

$$\frac{z}{z_x} + \frac{x}{x_z} - 1 = 0 \tag{21_y}$$

Now $\dfrac{x_b}{x_y} + \dfrac{y_a}{y_x} - 1 = \dfrac{1}{\beta_a^{\,2}} + \dfrac{1}{\alpha_b^{\,2}} - 1 = \alpha_b^{\,2} + \beta_a^{\,2} - \alpha_b^{\,2}\beta_a^{\,2} = 0$

and (21_x) or (21_y) can always be satisfied for some value of z; therefore ($20_z^{\,1}$) intersects the chord. In the same manner it may be proved that ($20_x^{\,1}$) and ($20_y^{\,1}$) intersect the chord. It follows, then, that (20_z), (20_x), and (20_y) cannot intersect the chord, and hence belong to the same system of generation.

The equations of a pair of lines intersecting in a given point of the hyperboloid and belonging to different systems of generation can be easily found by the condition that one of them must intersect (20) and the other (20^1). I omit this, but give a remarkable symmetrical form of the equation of the hyperboloid:

$$\begin{aligned} 0 &= (x - x_b)(y - y_c)(z - z_a) - (x - x_c)(y - y_a)(z - z_b) \qquad (22) \\ &= x(y_c z_a - y_a z_b) + y(z_a x_b - z_b x_c) + z(x_b y_c - x_c y_a) \\ &\quad - xy(z_a - z_b) - yz(x_b - x_c) - zx(y_c - y_a), \text{ because } x_b y_c z_a = x_c y_a z_b \\ &\qquad \text{by (18) and } (18^1). \end{aligned}$$

It is immediately evident that this equation is satisfied by equations (20). It is not uninteresting to prove that it also satisfies (21), or that it contains the chord, since it shows the remarkable pliability of these forms by virtue of the relations (16), (17), (18), (16^1), (17^1), (18^1).

The points (x_c, y_c, z_a), (x_c, y_c, z_b), (x_b, y_c, z_b), (x_b, y_a, z_b), (x_b, y_a, z_a), (x_c, y_a, z_a) form a warped hexagon, which lies wholly in the hyperboloid, and its sides may be considered six intersecting edges of a characteristic parallelopipedon. These edges are:

$$A = \frac{1}{2}(x_b - x_c)\,; \quad B = \frac{1}{2}(y_c - y_a)\,; \quad C = \frac{1}{2}(z_a - z_b) \tag{23}$$

and the co-ordinates of its center are:

$$x_o = \frac{1}{2}(x_b + x_c)\,; \quad y_o = \frac{1}{2}(y_c + y_a)\,; \quad z_o = \frac{1}{2}(z_a + z_b) \tag{24}$$

and these must be those of the center of the hyperboloid also.

Transferring the origin of co-ordinates to this center, we have the equation of the hyperboloid regarding (23):

$$0 = (x - A)(y - B)(z - C) - (x + A)(y + B)(z + C) \tag{25}$$

From this equation we soon find by familiar processes the lengths and directions of the principal axes.

As to the question, Which of the alignment curves should be used in geodesy? I observe that between two intervisible points on the terrestrial spheroid the difference between the course of these curves is so extremely minute that they are practically identical; we can use then that method of tracing which is most convenient. For the distance of non-intervisible stations I consider the brachisthode the *geodetic* line as heretofore, because 1st, the diorthode becomes impracticable; and 2d, it cannot be divided into portions which are themselves diorthodes. As Assistant Wm. Eimbeck, of the United States Coast and Geodetic Survey, suggested to me, the diorthode proper cannot even be traced between very distant stations, which are intervisible only from very elevated positions, such as high peaks or the usual wooden structures. This led me to consider a new class of alignment curves—the apparent horizon alignment curves. The *a. h.* pro-orthode would be the locus of all points for which the tangent cuts the normal at the forward end; while the *a. h.* diorthode is a curve, at any point of which a tangent to the surface, which passes through the normal at one end, also passes through that at the other end. The equation (3) being adapted to these changed conditions will furnish also the equations of these curves; and I have thus found that the *a. h.* diorthode on an ellipsoid has an intersecting surface of the fourth order.

Messrs. HARKNESS and DOOLITTLE made remarks on this paper.

Mr. ASAPH HALL then made a communication on

THE DETERMINATION OF THE MASS OF A PLANET FROM OBSERVATIONS OF TWO SATELLITES.

[Abstract.]

M. Struve recommends that the position angle and distance of one satellite from another satellite be measured, instead of referring the place of each to the center of the primary planet; and a series of such measurements on satellites of Jupiter has been begun under his direction at Pulkowa. These observations are found to occupy one-third the time, and are considered two or three times as accurate as those where the planet is used. The most important advantage of the new method is its freedom from the unknown constant errors attending the old, due to the great difference in size and bright-

ness of the objects measured. The price to be paid for this advantage is a greatly increased complexity in the computation; for the elements of both orbits now enter into each equation of condition, and there are therefore twelve normal equations instead of six to solve. The comparative difficulty may be estimated by the number of auxiliary quantities that must be computed in the solution of n equations, namely:

$$\frac{1}{6} n (n + 1) (n + 5),$$

which amounts to 77 for $n = 6$, and to 442 for $n = 12$; a value nearly six times as great. But it is worth while to bear in mind that the twelve equations, by giving the elements and mean distance of each satellite, give two values of the planet's mass.

Mr. HARKNESS called attention to the advantage of substituting an accidental error, be it even a large one, for an unknown constant error.

Mr. TAYLOR criticised the designations usually given to the apsides of satellites orbits as being particular when they should be general. He suggested the terms *peri-apsis* and *apo-apsis*, or *aphapsis*.

Remarks were also made by Messrs. KUMMELL and HILL.

Before adjournment the Chairman replied to some questions as to the new object glass for the Imperial Observatory at Pulkowa; and gave a short explanation of the difficulty of calculating the true anomaly in elliptic orbits.

4TH MEETING. MAY 9, 1883.

The Chairman presided.

Present: twelve members and one guest.

The report of a committee appointed by the General Committee of the Society to consider matters pertaining to Sections was read.

Mr. DOOLITTLE read a paper entitled

INFINITE AND INFINITESIMAL QUANTITIES.

[Abstract.]

An infinitesimal may be defined as the result of infinite division;

but the term *infinite division* probably does not represent the same conception to all mathematicians. If we suppose a quantity divided into a number of parts, and each of these parts subdivided, and similar subdivisions to go on forever, each requiring finite time, we have a conception to which the name *infinite division* may be given with some appropriateness, but which might better be called *eternal division.* Such division never reaches a result. But if we suppose the time of each subdivision to be proportional to the magnitude of each part, the entire process is completed in finite time, although no limit can be given to the number of subdivisions. If a point be supposed to have passed with constant velocity over a given distance, there was a time when it had passed over half the distance; afterward a time when the remaining distance was one-fourth of the original distance; the number of such successive halvings is certainly unlimited; and the result is that there is no remaining distance. This is division infinite but not eternal, and the result seems to be zero.

As a point is defined to be position without magnitude, so may an infinitesimal be defined to be quantitative relation without magnitude. The terms *infinitesimal, differential, nothing,* and *zero,* are not synonyms. They have the same logical denotation but differ in connotation. Mathematicians usually speak of "the value" or "the true value" of a vanishing fraction, as though any quantity whatever were not a true value. The term *serial value* is proposed as conducive to clearness of thought. A differential coefficient is the serial value of a vanishing fraction; and a differential or infinitesimal may be further defined as zero in serial relation to continuously diminishing quantity.

The term *infinitesimal* is however frequently employed like other terms to denote the symbol of its exact signification. We speak of drawing and erasing lines, meaning the visible symbols of Euclidean lines. Even in our purely mental processes we give the name *points* to the imagined small volumes that symbolize positions without magnitude. In like manner the term *infinitesimal* is employed to denote the imagined small quantity in approximate relation that symbolizes a relation which becomes exact only when magnitude disappears.

A line is infinite relatively to a point, but infinitesimal, *i. e.*, zero, relatively to a surface or volume. Every quantity is finite relatively to other quantities of its own order—zero relatively to orders

above and infinite relatively to orders below. A volume is integrated from surfaces, a surface from lines, and a line from points. Each integral is infinite relatively to the magnitudes from which it is integrated. As momentum is integrated from motion-generating force, it is infinite relatively thereto. Momentum may also be dissipated by infinitesimal decrements; and it is possible that momentum is always thus dissipated and re-integrated whenever motion is communicated from one body to another; but the principles of mathematics are equally consistent with the hypothesis that actual contact sometimes occurs, in which case motion is directly and instantaneously transmitted without dissipation or re-integration. Granting that infinitesimal time requires infinite force, momentum satisfies that condition.

This paper gave rise to considerable discussion, in which Messrs. TAYLOR, HILL, KUMMELL, and LEFAVOUR maintained the legitimacy of the notion of infinitesimals as real elements out of which quantity is built up; Messrs. ELLIOTT, DOOLITTLE, and FARQUHAR took the opposite ground, preferring the Newtonian view of the Calculus; while Mr. CHRISTIE, while preferring the infinitesimal method, maintained that no evaluation of continuous quantity, in terms of units as it must necessarily be, could ever be precise or entirely satisfactory, to however small a compass the uncertainty be reduced. Mr. CHRISTIE also pointed out some paradoxes to which the usual definitions of curves and tangents appeared to lead.

Mr. ELLIOTT then exhibited some tables to serve as a perpetual calendar, and gave a full explanation how by means of them the day of the week corresponding to that of the month for any year, New or Old Style, B. C. or A. D., could be found.

5TH MEETING. MAY 23, 1883.

The Chairman presided.

Twenty members and guests present.

The appointment of the committee called for under the new Standing Rule relating to papers read before Sections of the Society was considered. Mr. TAYLOR moved that the committee consist of the Chairman and Secretary and a third member to be

appointed by the Chair. After some discussion by Messrs. HARKNESS and ELLIOTT it was so ordered, with the additional provision that this appointment be made for each paper separately.

Mr. G. W. HILL made a communication on

PLANETARY PERTURBATIONS OF THE MOON,

which was yet unfinished when he yielded the floor to Mr. G. K. GILBERT, who made a communication on

GRAPHIC TABLES FOR COMPUTING ALTITUDES FROM BAROMETRIC DATA.

This paper will appear in the Bulletins of the U. S. Geological Survey.

6TH MEETING. JUNE 6, 1883.

The Chairman presided.

Present, sixteen members and guests.

Mr. G. W. HILL concluded his paper on

CERTAIN POSSIBLE ABBREVIATIONS IN THE COMPUTATION OF THE LONG-PERIOD PERTURBATIONS OF THE MOON'S MOTION DUE TO THE DIRECT ACTION OF THE PLANETS.

[Abstract.]

Hansen has characterized the calculation of these inequalities as extremely difficult. However, it seems to me that if the shortest methods are followed there is no ground for such an assertion. The work may be divided into two portions independent of each other. In one the object is to develop, in periodic series, certain functions of the moon's coördinates, which in number do not exceed five. This portion is the same whatever planet may be considered to act, and hence may be done once for all. In the other portion we seek the coefficients of certain terms in the periodic development of certain functions, five also in number, which involve the coördinates of the earth and planet only. And this part of the work is very similar to that in which the perturbations of the earth by the planet in question are the things sought. And as the multiples of the mean motions of these two bodies, which enter into the expres-

sion of the argument of the inequalities under consideration, are necessarily quite large, approximate values of the coefficients may be obtained by semi-convergent series similar to the well-known theorem of Stirling. This matter was first elaborated by Cauchy,* but in the method as left by him we are directed to compute special values of the successive derivatives of the functions to be developed. Now it unfortunately happens that these functions are enormously complicated by successive differentiation, so that it is almost impossible to write at length their second derivatives. Manifestly then, it would be a great saving of labor to substitute for the computation of special values of these derivatives a computation of a certain number of special values of the original function, distributed in such a way that the maximum advantage may be obtained. This modification has given rise to an elegant piece of analysis.

It will be noticed that in this method it is necessary to substitute in the formulæ, from the outset, the numerical values of the elements of the orbits of the earth and planet. There seems to be no objection to this on the practical side, as for the computation of the inequalities sought no partial derivatives of R, with respect to these elements, are required.

The paper is printed in full in the *American Journal of Mathematics,* Vol. VI.

Mr. E. B. ELLIOTT made a communication on

UNITS OF FORCE AND ENERGY, INCLUDING ELECTRIC UNITS.

SEVENTH MEETING. NOVEMBER 21, 1883.

The Chairman presided.

Thirteen members present.

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* Mémoire sur les approximations des fonctions de très-grands nombres, and Rapport sur un Mémoire de M. Le Verrier, qui a pour objet la détermination d'une grande inégalité du moyen mouvement de la planète Pallas. Comptes Rendus de l'Académie des Sciences de Paris. Tom. XX, pp. 691–726, 767–786, 825–847.

Mr. C. H. KUMMELL read a communication entitled

THE THEORY OF ERRORS PRACTICALLY TESTED BY TARGET-SHOOTING.

[Abstract.]

Sir John Herschel treats a special case in which shots of equal probability are in circles. According to Liagre's theory target shooting is compounded of two distinct operations, viz., sighting and leveling, each of which is liable to errors, independently following the ordinary linear law of error. Some reasons for the independence of these operations are that for sighting the direction of the wind, which does not affect the leveling, must be regarded; and that, on the other hand, leveling only is affected by the range. The consequences of Liagre's theory will now be developed.

Let $x =$ error of sighting and ε_x its mean error;

$y =$ error of leveling and ε_y its mean error;

then it follows that

$$\frac{dx}{\varepsilon_x\sqrt{2\pi}}\, e^{-\frac{x^2}{2\varepsilon_x^2}} = \text{probability to hit anywhere at distance } x \text{ from sighting axis.} \tag{1_x}$$

$$\frac{dy}{\varepsilon_y\sqrt{2\pi}}\, e^{-\frac{y^2}{2\varepsilon_y^2}} = \text{probability to hit anywhere at distance } y \text{ from leveling axis.} \tag{1_y}$$

$$\therefore \frac{dxdy}{2\varepsilon_x\varepsilon_y\pi}\, e^{-\frac{x^2}{2\varepsilon_x^2}-\frac{y^2}{2\varepsilon_y^2}} = \text{probability to hit the point } (x, y). \tag{2}$$

This probability is the same for any point on the ellipse:

$$\frac{x^2}{\varepsilon_x^2} + \frac{y^2}{\varepsilon_y^2} = \frac{r^2}{\varepsilon^2}, \text{ where } \varepsilon^2 = \frac{1}{2}(\varepsilon_x^2 + \varepsilon_y^2) \tag{3}$$

This I shall call, then, an equal probability ellipse; its semi-axes are:

$$\frac{\varepsilon_x}{\varepsilon}\, r \text{ and } \frac{\varepsilon_y}{\varepsilon}\, r \tag{4}$$

and $r =$ mean semi-diameter (which is equal to its conjugate).

$$\text{Assume} \qquad x_r = \frac{\varepsilon}{\varepsilon_x} \qquad \text{and} \qquad y_r = \frac{\varepsilon}{\varepsilon_y}\, y \tag{5}$$

then every point on the equal probability ellipse (3) corresponds to a point (x_r, y_r) on the circle: $x_r^2 + y_r^2 = r^2$, (6)
which is the reduced equal probability circle.

Counting directions from the right of the x - axis, let

$$a = \text{direction of } (x, y) \tag{7}$$

$$a_r = \text{" " } (x_r, y_r), \text{ or reduced direction of } (x, y) \tag{8}$$

then $\tan a = \frac{y}{x} = \frac{\varepsilon_y}{\varepsilon} y_r \div \frac{\varepsilon_x}{\varepsilon} x_r = \frac{\varepsilon_y}{\varepsilon_x} \tan a_r$ (9)

also $$x = \frac{\varepsilon_x}{\varepsilon} r \cos a_r \tag{10$_x$}$$

$$y = \frac{\varepsilon_y}{\varepsilon} r \sin a_r \tag{10$_y$}$$

whence $dx = \frac{\varepsilon_x}{\varepsilon} \cos a_r \, dr - \frac{\varepsilon_x}{\varepsilon} r \sin a_r da_r$

$$dy = \frac{\varepsilon_y}{\varepsilon} \sin a_r dr + \frac{\varepsilon_y}{\varepsilon} r \cos a_2 da_r$$

Transforming, then, (2) to the new variables, r and a_r, we must replace:

$$dx\, dy \text{ by } \frac{\varepsilon_x \varepsilon_y r dr da_r}{\varepsilon^2}$$

and thus obtain

$\frac{r dr da_r}{2\pi\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}}$ = probability to hit a point of which (r, a_r) is the reduced point. (11)

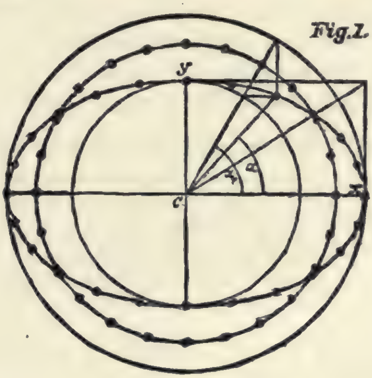


Fig. 1.

Fig. 1 exhibits 24 shots of equal probability, on an equal probability ellipse, and their reduced positions evenly distributed over the reduced circle.

The probability to hit anywhere on the perimeter of an equal probability ellipse of mean semi-diameter, r, is found by integrating (11), with respect to α_r, through a circumference. It is

$$P_r = \frac{rdr}{\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}} \tag{12}$$

Let n_r = number of shots on area of equal probability ellipse of semi-diameter r, and n = total number; then

$$\frac{n_r}{n} = \int_o^r \frac{rdr}{\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}} = 1 - e^{-\frac{r^2}{2\varepsilon^2}} \text{ or } \frac{n - n_r}{n} = e^{-\frac{r^2}{2\varepsilon^2}} \tag{13}$$

Let $r = \rho$; if $n_r = \frac{1}{2}n$, then $\frac{1}{2} = e^{-\frac{\rho^2}{2\varepsilon^2}} \therefore \rho = \varepsilon\sqrt{2l2}$ (14)

The ellipse:

$$\frac{x^2}{\varepsilon_x^2} + \frac{y^2}{\varepsilon_y^2} = 2l2 \tag{15}$$

is then an even chance ellipse, which is hit or missed with equal probability. Eliminating ε between (13) and 14), we obtain:

$$\left(\frac{n - n_r}{n}\right)^{\rho^2} = \left(\frac{1}{2}\right)^{r^2} \tag{16}$$

$$\therefore \rho = r\sqrt{\frac{log\, 2}{log\left(\frac{n}{n - n_r}\right)}} \tag{17}$$

These formulæ agree with Herschel's in form, and have, also, the same signification, in case the precisions of sighting and leveling are equal, for in that case the ellipses (3) and (15) become circles and r, ρ their radii, respectively. Herschel employs these formulæ for determining the skill of a marksman, which he defines to be $= \frac{1}{\rho}$, from the number of shots that have fallen on a circle of radius r.

Correspondingly, we should have to count the shots that have fallen on an equal probability ellipse, the axes of which have the unknown ratio $\frac{\varepsilon_y}{\varepsilon_x}$, which, as yet, we have no method of finding; therefore formulæ (14) and (17) cannot be employed in their general signification. If, nevertheless, we count the shots on a circle of radius r and compute a value for ρ and ε, we shall come as near to their true values as the problem requires, especially if the precisions of sighting and leveling are not very different. This can be

shown analytically by proving that the probability of hitting the area of the circle

$$x^2 + y^2 = r^2$$

differs from that of hitting the equal probability ellipse

$$\frac{x^2}{\varepsilon_x^{\ 2}} + \frac{y^2}{\varepsilon_y^{\ 2}} = \frac{r^2}{\varepsilon^2}$$

by terms of the fourth order, with respect to the difference between the mean errors of sighting and leveling.

In computing ρ by (17) the radius (or mean semi-diameter) r is left arbitrary; it is, however, not at all indifferent; for if we take it very small or very large it will give very unreliable values of ρ.

There must then be a certain magnitude of r giving the most reliable value of ρ, and it is that which makes $P_r = \frac{rdr}{\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}}$ a maximum. This gives the condition: $0 = \frac{1}{\varepsilon^2} - \frac{r^2}{\varepsilon^4} \therefore r = \varepsilon$

Thus the most favorable value of r for determining ρ is the mean error ε and the ellipse

$$\frac{x^2}{\varepsilon_x^{\ 2}} + \frac{y^2}{\varepsilon_y^{\ 2}} = 1 \qquad (18)$$

is the ellipse of the most probable shot.

Placing $r = \varepsilon$ in (13), we have

$$\frac{n - n_\varepsilon}{n} = e^{-\frac{1}{2}} = 0.60653 \ldots .$$

$$\therefore \ n_\varepsilon = \left(1 - e^{-\frac{1}{2}}\right) n = 0.39347 \ldots n = 0.4n \text{ nearly} \qquad (19)$$

The most probable shot is, therefore, the distance of the $(0.4n)$th shot from the center nearly; also the mean of the $(0.4n + m)$th, and the $(0.4n - m)$th shot should, if m is not too large, give a fair value of the most probable shot.

Solving (13) for ε, we have also

$$\varepsilon = \frac{r}{\sqrt{2l \frac{n}{n - n_r}}} \qquad (20)$$

From the definition of ε_x and ε_y it is obvious that

$$\varepsilon_x = \sqrt{\frac{[x^2]}{n}}; \ \varepsilon_y = \sqrt{\frac{[y^2]}{n}} \qquad (21)$$

which formulæ afford a comparison between the precisions of sighting and leveling. We have then

$$\varepsilon = \sqrt{\frac{[s^2]}{2n}} \text{ if } s = \sqrt{x^2 + y^2} \tag{22}$$

This formula, although laborious for practical use, is the most rigorous measure of skill in shooting, and there is no need of other formulæ except when shots are lost. In that case it requires an important modification, whereby it loses in rigor if the number of lost shots is considerable. Assuming the precisions of sighting and leveling equal, then the reduced distance r in (12) will be the actual distance s of a shot; and if the target is circular, of limiting radius R, we have

$$[s^2]_1^{n_R} = n \int_0^R s^2 \frac{s\,ds}{\varepsilon^2} e^{-\frac{s^2}{2\varepsilon^2}}$$

$$= n \left\{ - R^2 e^{-\frac{R^2}{2\varepsilon^2}} + 2\varepsilon^2 \left(1 - e^{-\frac{R^2}{2\varepsilon^2}} \right) \right\}$$

Now by (13) $$n_R = n \left(1 - e^{-\frac{R^2}{2\varepsilon^2}} \right)$$

therefore $$[s^2]_1^{n_R} = 2n_R \varepsilon^2 - \left(n - n_R \right) R^2$$

and $$\varepsilon^2 = \frac{[s^2]_1^{n_R} + \left(n - n_R \right) R^2}{2n_R} \tag{23}$$

This formula reverts, of course, to (22), if $n = n_R$, and it makes the most probable sum of the squares of the lost shots

$$[s^2]_{n_R}^{n} = \frac{n}{n_R} \left(n - n_R \right) R^2$$

and since $\left(n - n_R \right) R^2$ is the smallest possible actual value of this quantity; this expression for it is quite plausible.

The targets used by the National Rifle Association are rectangular. (At long range they are 12 feet wide and 6 feet high).

Let a (= 6 feet) be the limiting value of x and b (= 3 feet) that for y, then we have, if n_{ab} is the number of hitting shots

$$n_{ab} = n \int_{-a}^{a} \frac{dx}{\varepsilon_x \sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \int_{-b}^{b} \frac{dy}{\varepsilon_y \sqrt{2\pi}} e^{-\frac{y^2}{2\varepsilon_y^2}} = n\, Pt_a Pt_b \qquad (24)$$

The integral $Pt_a = \int_{-a}^{a} \frac{dx}{\varepsilon_x \sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} = \frac{2}{\sqrt{\pi}} \int_0^{t_a} dt\, e^{-t^2}$, and

similarly Pt_b, is tabulated in Chauvenet's Method of Least Squares (Table IX, appendix, to the argument t), and is therefore known.

We have further:

$$[x^2]_1^{n_{ab}} = n \int_{-a}^{a} x^2 \frac{dx}{\varepsilon_x \sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \int_{-b}^{b} \frac{dy}{\varepsilon_y \sqrt{2\pi}} e^{-\frac{y^2}{2\varepsilon_y^2}}$$

$$= nPt_b \left\{ -\left[\frac{\varepsilon_x x}{\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \right]_{-a}^{a} + \varepsilon_x^2 \int_{-a}^{a} \frac{dx}{\varepsilon_x \sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \right\}$$

$$= n\varepsilon_x^2 Pt_b\, (Pt_a - t_a P't_a) \qquad (25_x)$$

Here $P't_a$ denotes $\frac{dPt_a}{dt_a} = \frac{2}{\sqrt{\pi}} e^{-t_a^2}$ and can also be taken from Chauvenet's table, being 100 $\times$ difference. Similarly,

$$[y^2]_1^{n_{ab}} = n\varepsilon_y^2 Pt_a\, (Pt_b - t_b P't_b) \qquad (25_y)$$

By virtue of (24) we have also

$$\varepsilon_x^2 = \frac{[x^2]_1^{n_{ab}}}{n_{ab}\left(1 - t_a \frac{P't_a}{Pt_a}\right)} \qquad (25_x')$$

$$\varepsilon_y^2 = \frac{[y^2]_1^{n_{ab}}}{n_{ab}\left(1 - t_b \frac{P't_b}{Pt_b}\right)} \qquad (25_y')$$

and these formulæ may be used to compute ε_x and ε_y by an obvious approximative process. They show that $\varepsilon_x^2 > \frac{[x^2]_1^{n_{ab}}}{n_{ab}}$, as it should

be; but it may, or rather must, happen sometimes that the most probable increase of the sum of x^2 and y^2 or $[x^2]_{n_{ab}}^{n} + [y^2]_{n_{ab}}^{n}$ consistent with (25′) is $< (n - n_{ab})\, b^2$, b being the smaller limit. Such a result cannot be accepted, being contradictory to the fact that there are $n - n_{ab}$ shots at a greater distance than b. The following method gives plausible results in that case. Assume

$$(\varepsilon_y^{\,2}) = \frac{[y^2]_{\mathrm{I}}^{n_{ab}} + (n - n_{ab})b^2}{n} \quad (b<a) \qquad (25_y'')$$

as first approximate value in $(25_y')$, and if $\varepsilon_y < (\varepsilon_y)$ adopt (ε_y) as final value of ε_y: but if $\varepsilon_y > (\varepsilon_y)$, then proceed in approximating to ε_y by $(25_y')$. The solution of $(25_x')$ gives, as heretofore, the best value of ε_x. Among the target records of the international shooting match of 1874, at Creedmoor, there are 9 with lost shots, 5 of which give too small an increase of sum of squares, and this means that from the record of the hitting shots it would not appear probable that so many shots were lost.

Instead of the squares, we may, however, employ first powers of distances; and I shall develop the requisite formulæ for a circular target and equal precisions.

We have $$[s]_1^{n_R} = n\int_o^R s\,\frac{sds}{\varepsilon^2}\,e^{-\frac{s^2}{2\varepsilon^2}}$$

$$= n\left(-Re^{-\frac{R^2}{2\varepsilon^2}} + \varepsilon\sqrt{\frac{\pi}{2}}\,Pt_R\right)$$

$$= -\left(n - n_R\right)R + n\varepsilon\sqrt{\frac{\pi}{2}}\,Pt_R \qquad \text{by (13)}$$

$$\therefore \varepsilon = \frac{[s]_1^{n_R} + \left(n - n_R\right)R}{nPt_R}\sqrt{\frac{2}{\pi}} \qquad (26)$$

If $n_R = n$, this becomes $$\varepsilon = \frac{[s]}{n}\sqrt{\frac{2}{\pi}} \qquad (27)$$

The quantity $$r_0 = \frac{[s]}{n} = \varepsilon\sqrt{\frac{\pi}{2}} \qquad (28)$$

which may be called the *average shot*, has been recently introduced by the United States Ordnance Department, under the name "radius of the circle of shots," in place of the extremely defective quantity, the mean absolute deviation, the insufficiency of which was pointed out by Henry Metcalfe, Captain of Ordnance, in the Report of the Chief of Ordnance of 1882. Thus the adopted method of discussion of the precision of firearms, as used by that department, is in agreement with Liagre's theory, only the shots are not referred to the true center, but to the "center of shots," viz.: their center of gravity.

We have, now, the following three quantities, each of which may be used as a measure of precision, sighting and leveling being equally good.

1, the even chance shot, ρ.

2, the most probable shot, ε, (or mean error of sighting and leveling).

3, the average shot, r_0, also called radius of the circle of shots; and they are related to each other as follows:

$$\frac{\rho}{\sqrt{2l2}} = \varepsilon = r_0\sqrt{\frac{2}{\pi}} \tag{29}$$

The preceding formulæ I regard as complete, for practical discussion of target records, provided there is no evidence for a constant vitiating cause. If, for example, during a shooting match the wind is blowing constantly in the same direction, the effect of this might be partially revealed by computing for the whole match the quantity:

$$x_0 = \frac{[x]}{n} \tag{30}$$

If the sign of this quantity is consistent with the observed direction of the wind, it might, perhaps, be proper to refer the shots to a new center, to the right or left of the true center, by this quantity. In that case we have, however,

$$\varepsilon_x = \sqrt{\frac{[x^2] - nx_0^{\,2}}{n-1}} \tag{31}$$

In leveling there may be a somewhat constant individual habit of holding too high or too low, which, however, ought not to be eliminated in a fair discussion of a match, although it would be of interest to compute the quantity

$$y_0 = \frac{[y]}{n}$$

for each marksman and for a whole team.

Much less proper, it would seem to me, to regard the position of the axes unknown, and to compute their most probable position. If center and axes are to be determined, x' y' denote the co-ordinates of a shot from a random origin and position of axes, and w the angle of turning the latter into their most probable direction; then the most probable co-ordinates of a shot are:

$$x = x_0 + x' \cos w + y' \sin w; \quad y = y_0 + y' \cos w - x' \sin w.$$

Imposing the conditions of a minimum for $[x^2]$ and $[y^2]$, we find

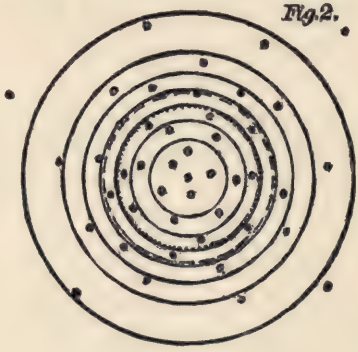
$$\left.\begin{aligned} x_0 &= -\frac{1}{n}\Big([x'] \cos w + [y'] \sin w\Big); \\ y_0 &= -\frac{1}{n}\Big([y'] \cos w - [x'] \sin w\Big) \end{aligned}\right\} \quad (33)$$

$$\tan 2w = \frac{[x'y'] - \frac{1}{n}[x'][y']}{[x'^2] - \frac{1}{n}[x']^2 - [y'^2] + \frac{1}{n}[y']^2} \quad (34)$$

These formulæ have, however, their proper place in the theory of Andræ's "Fehler-ellipse."

Fig. 2 exhibits an ideal distribution of 45 shots. Each ring contains 6 shots, leaving 3 shots between the outer ring and infinity. The dotted circle is that of the most probable shot, and the dashed one that of the even chance shot.

Fig. 2.



The following table refers to the combined target record of the Irish team at 800 yards range, in the international shooting match of 1874, at Creedmoor:

Irish Team at 800 *yards:* $\varepsilon = 1.3095$ *ft.;* 90 *shots,* 88 *hits.*

	Radii.	No. of shots on circle.		Discrepancy.	No. of shots on ring.		Discrepancy.
		Theory.	Actual.		Theory.	Actual.	
	Feet.						
	0.5	6.3	5	+1.3	6.3	5	+1.3
	1.0	22.8	22	+0.8	16.5	17	—0.5
	1.5	43.3	47	—3.7	20.5	25	—4.5
	2.0	62.0	58	+4.0	18.7	11	+7.7
	2.5	75.5	74	+1.5	13.5	16	—2.5
Leveling limit--	3.0	83.5	83	+0.5	8.0	9	—1.0
	3.5	87.5	87+ ?	+0.5?	4.0	4+ ?	0.0?
	4.0	89.2	87+ ?	?	1.7	?	?
	4.5	89.8	88+ ?	?	0.6	1+ ?	—0.4?
					89.8	88+2	

A target of 50 pistol shots at 50 yards range shows similar discordance between theory and practice, which, on an average, may be taken less than 5 per cent.

Target of pistol shots at 50 yards range: $\varepsilon = 0.167$ *ft.;* 50 *shots, no misses.*

Radii.	No. of shots on circle.		Discrep'y.	No. of shots on ring.		Discrep'y.
	Theory.	Actual.		Theory.	Actual.	
in.						
0.5	1.5	1	+0.5	1.5	1	+0.5
1.0	5.9	8	—2.1	4.4	7	—2.6
1.5	12.2	14	—1.8	6.3	6	+0.3
2.0	19.7	23	—3.3	7.5	9	—1.5
2.5	27.0	28	—1.0	7.3	5	+2.3
3.0	33.7	33	+0.7	6.7	5	+1.7
3.5	39.6	37	+2.6	5.9	4	+1.9
4.0	43.2	41	+2.2	3.6	4	—0.4
4.5	46.0	46	0.0	2.8	5	—2.2
5.0	47.8	47	+0.8	1.8	1	+0.8
5.5	48.8	49	—0.2	1.0	2	—1.0
6.0	49.4	50	—0.6	0.6	1	—0.4
				49.4	50	

Mr. Elliott gave an example of remarkably close agreement between the distribution of errors by theory and by observation of the chest measurements of 1,516 United States soldiers, reported by Dr. Bulkley at the Berlin Statistical Congress. In five groups the greatest difference was four-tenths per cent.

EIGHTH MEETING. DECEMBER 5, 1883.

The Chairman presided.

Fourteen members and guests present.

Mr. ALVORD discussed

A SPECIAL CASE IN MAXIMA AND MINIMA,

the problem being to find the radius of the sphere that will displace the maximum quantity of liquid from a conical wine glass full of water.

The differential co-efficient, when put equal to zero, is in the form of two factors. Equating each to zero, one gives the radius of the maximum sought; the other gives a still larger radius, which proves to be the radius of the sphere just tangent to the centre of the base of the cone, and to the sides of the cone, extended upwards. This gives the minimum displacement equal to zero. Calling a the radius of the base, b the height, and c the slant height of the cone, theradius of the sphere producing maximum displacement equals $\frac{abc}{(c-a)(2a+c)}$; the radius corresponding to minimum displacement equals $\frac{ab}{c-a}$.

When the radius is still greater, the sphere does not reach the surface of the liquid, but displaces an imaginary quantity of the same. An analytical expression for this case was sought in vain; the result above is simple, and no square root of a negative quantity appears. By some device in the mode of investigation, this imaginary case might appear, as in the question to obtain the radical axis of two circles, discussed by Salmon.

Mr. KUMMELL suggested that the close relation between the circle $x^2 + y^2 = R^2$ and the equilateral hyperbola $x^2 - y^2 = R^2$, each of which could be regarded as an imaginary branch of the other, might help us to understand many of such difficulties. He showed that the radical axis of two circles not intersecting was the common chord of two equilateral hyperbolas whose major axes were those diameters of the circles which lie in the same straight line.

Mr. ELLIOTT read a communication on

A FINANCIAL PROBLEM,

in which he gave formulæ for calculating the advantage of in-

vestment in United States Government bonds, at six or at four per cent., and making use of the banking privileges thus available, over investment at a higher rate without such privileges. The restrictions caused by the high premium on Government bonds, the bank tax, and the necessary specie reserve were all allowed for.

This paper was discussed by Messrs. HARKNESS, DE LAND, SMILEY, and others.

Mr. H. FARQUHAR presented the following

FORM OF LEAST-SQUARE COMPUTATION.

Suppose four unknown constants, A, B, C and D, are to be calculated from equations of condition of the form

$$a\mathrm{A} + b\mathrm{B} + c\mathrm{C} + d\mathrm{D} = y.$$

Arrange columns in order (1) a^2, (2) ab, (3) ac, (4) ad, (5) ay, (6) b^2, (7) bc, (8) bd, (9) by, (10) c^2, (11) cd, (12) cy, (13) d^2, (14) dy.

Add up first five columns and place under (2) to (5) the quotients of their sums divided by $\Sigma(1)$.

Put the product $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(2)$ under (6), $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(3)$ under (7), $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(4)$ under (8), $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(5)$ under (9), $\frac{\Sigma(3)}{\Sigma(1)}\Sigma(3)$ under (10), $\frac{\Sigma(3)}{\Sigma(1)}\Sigma(4)$ under (11), $\frac{\Sigma(3)}{\Sigma(1)}\Sigma(5)$ under (12), $\frac{\Sigma(4)}{\Sigma(1)}\Sigma(4)$ under (13), and $\frac{\Sigma(4)}{\Sigma(1)}\Sigma(5)$ under (14), *reversing the sign in every case.*

Then add up (6) to (9), placing under the sums of (7) to (9) their quotients divided by $\Sigma'(6)$.

Put the product $\frac{\Sigma'(7)}{\Sigma'(6)}\Sigma'(7)$ under (10), $\frac{\Sigma'(7)}{\Sigma'(6)}\Sigma'(8)$ under (11), $\frac{\Sigma'(7)}{\Sigma'(6)}\Sigma'(9)$ under (12, $\frac{\Sigma'(8)}{\Sigma'(6)}\Sigma'(8)$ under (13), and $\frac{\Sigma'(8)}{\Sigma'(6)}\Sigma'(9)$ under (14), *reversing each sign.*

Add (10) to (12), putting quotients $\frac{\Sigma''(11)}{\Sigma''(10)}$ and $\frac{\Sigma''(12)}{\Sigma''(10)}$ under the sums.

Put the product $\frac{\Sigma''(11)}{\Sigma''(10)}\Sigma''(11)$ under (13) and $\frac{\Sigma''(11)}{\Sigma''(10)}\Sigma''(12)$ under (14), *reversing the signs.*

Add (13) and (14); when $\frac{\Sigma'''(14)}{\Sigma'''(13)} = \text{D}$.

Then, under (12), enter $\frac{\Sigma''(12)}{\Sigma''(11)} - \frac{\Sigma''(11)}{\Sigma''(10)}\text{D} = \text{C}$.

Next, under (9), enter $\frac{\Sigma'(9)}{\Sigma'(6)} - \frac{\Sigma'(8)}{\Sigma'(6)}\text{D} - \frac{\Sigma'(7)}{\Sigma'(6)}\text{C} = \text{B}$.

Lastly, under (5), enter $\frac{\Sigma(5)}{\Sigma(1)} - \frac{\Sigma(4)}{\Sigma(1)}\text{D} - \frac{\Sigma(3)}{\Sigma(1)}\text{C} - \frac{\Sigma(2)}{\Sigma(1)}\text{B} = \text{A}$.

NOTES.—[1] The sign of summation is distinguished by an additional stroke for every additional quantity introduced under the column added up.

[2] These additional quantities, under the columns of squares, (6), (10), and (13), will evidently all be negative.

[3] This form may be extended to any number of unknown quantities, by insertion of *ae*, etc., between (4) and (5), *be*, etc., between (8) and (9), and so on. Modifications where there is a smaller number of unknown constants, and where one of them has the coefficient always unity, will be obvious.

[4] One of the quantities *a*, *b*, etc., will, in many computations, be zero when another one is significant, and *vice-versa*; as when one unknown quantity changes in the course of a series of observations. In this case we may save some columns by arranging our equation thus: $a_1\,\text{A}_1 + a_2\,\text{A}_2 + b\,\text{B} + \text{etc.} = y$ (where $a_1\,a_2 = 0$, always). Here two sums are found under columns (1) to (5), two quotients under (2) to (5), and two additional quantities placed under each of the other columns before they are summed up. The remainder of the work then proceeds as before, except that the *last* step will be duplicate.

[5] It will be found advisable always to make Σa, Σb, etc., as nearly zero as possible, so that the products will be smaller and there will be less danger of error.

[6] The computation is to be checked by applying A, B, etc., and finding the residuals of y. Then $\Sigma(a\,\Delta y)$, $\Sigma(a\,\Delta b)$, etc., should all be zero.

[7] Where but two unknown quantities are to be found, one of them with the constant coefficient unity (as $\text{A} + b\,\text{B} = y$), other methods will usually be preferable. Two of these will be given.

I. If the values of b are symmetrical, so that $b = \beta \pm b'_1, \beta \pm b'_2, \beta \pm b'_3$, etc., here all that is necessary to find B is to subtract the

value of y for every $\beta - b'$ from that for $\beta + b'$, to multiply the remainders by b', to find $\Sigma\,(b'\Delta y)$ and divide it by $2\,\Sigma\,(b'^2)$, when the quotient will be B. If A should be wanted also—as is very often not the case—then Σy must also be found, and $A = \frac{\Sigma y}{n} - \beta B$, where n equals the number of equations.

II. In all cases we may obtain the required values by taking the difference of b and of y from the mean of the column, multiplying the residual by the former difference, thus forming columns of $\left(b - \frac{\Sigma b}{n}\right)^2$ and $\left(b - \frac{\Sigma b}{n}\right)\left(y - \frac{\Sigma y}{n}\right)$ adding these and dividing the second sum by the first. That is,

$$B = \frac{\Sigma\left\{\left(b - \frac{\Sigma b}{n}\right)\left(y - \frac{\Sigma y}{n}\right)\right\}}{\Sigma\left\{\left(b - \frac{\Sigma b}{n}\right)^2\right\}}\text{; when } A = \frac{\Sigma y}{n} - \frac{\Sigma b}{n}B.$$

NINTH MEETING. DECEMBER 19, 1883.

The Chairman presided.

Sixteen members and guests present.

Mr. H. FARQUHAR furnished a

NOTE ON THE PROBLEM DISCUSSED BY MR. ALVORD,

in which he showed that the volume of a spherical segment of height h, $\pi h^2 (R - \frac{1}{3}h)$, being real for all values of h, both positive and negative, was to be interpreted for $h < 0$ or $h > 2R$ as the volume of the segment of the equilateral hyperboloid of two sheets whose axes equal R; this volume being taken with a negative sign. It was positive for negative values of h, since it must become zero when $h = 0$ by negative increments; hence the minimum of the function when $h = 0$ in such problems as the one discussed.

Mr. DOOLITTLE read a communication on

THE REJECTION OF DOUBTFUL OBSERVATIONS.

[Abstract.]

For the purposes of this discussion we may divide errors into

two grand classes, and name them, from their consequences, *instructive errors* and *uninstructive errors*. The latter class includes blunders in recording, pointing on wrong objects, &c. The former consists of errors that indicate error in other observations.

I once tried the experiment of dropping a short straight piece of wire five hundred times upon a sheet of ruled paper and counting the number of intersections of the wire with a ruled line. When the end of the wire touched or nearly touched a line, and intersection was doubtful, I counted it as half an intersection. I recorded the number of intersections in groups of fifty trials, as follows: 23, 26, 28.5, 24, 31.5, 28, 27, 14, 25, 28.5. These numbers may be regarded as observations from which may be deduced the probable ratio of the length of the wire to the distance between two consecutive lines; and it seems impossible to account for the remarkable smallness of the eighth number by any supposition of uninstructive error. It is almost certain that a ratio deduced from it alone is largely in error; but it indicates that the other nine observations are somewhat in error, and that its error is needed to counterbalance theirs. If we retain it, and regard the mean of all as the most probable truth, we infer that this observation is 11.55 units in error. If we reject it, and take the mean of the other nine as the most probable truth, we infer that this observation is 12 5-6 units in error. It should be remembered that the rejection of an observation does not sweep from existence the fact of its occurrence; but merely increases its already large estimate of error. Because an error of 11.55 units is so large as to be very improbable, shall we therefore infer that an error of 12 5-6 units is more probable?

It seems very clear to me that the larger an instructive error is the more instructive it is, and the more important is it that the observation containing it *should not* be rejected. The mean of all the ten above-described observations being regarded as the most probable truth, any one of the other nine could be better spared than the eighth. On the other hand, the larger an uninstructive error is, the more important it is that the observation *should* be rejected. Whenever an observation is intelligently rejected, there is a comparison of two antecedent probabilities, viz.: that of the occurrence of an instructive error of the magnitude involved and that of the occurrence of an uninstructive error of the same magnitude. When an error is evidently so large that it cannot possibly belong to the instructive class, the antecedent probability of such

an instructive error is 0; the antecedent probability of an uninstructive error is always greater than 0; and the observation should certainly be rejected. But since the theory of least squares allows no limit whatever to the possible magnitude of instructive errors, such rejection involves the admission that the method of least squares is not applicable to the case. When an observation involves a merely suspicious error, which is neither so large that instructiveness is impossible nor so small as to pass without question, it would seem reasonable that the observation should be weighted according to the relative magnitudes of the two antecedent probabilities which I have mentioned; but this can never be determined with any approach to mathematical precision.

In order to make this matter clear, let us suppose for example that ninety-nine observations of equal weight and known to be free from uninstructive error are separately written on as many cards; that the number 25 is arbitrarily written on a similar card; that these hundred cards are thoroughly shuffled; and that ten cards being then drawn at random, the following numbers appear on them: 15, 18, 14, 25, 17, 16, 15, 18, 16, 17. Let it be required to determine from these data, according to the theory of least squares, the probability that the number 25 on the fourth card drawn is the record of an observation. Here the antecedent probability of an uninstructive error is by hypothesis equal to 1-10.

I commence by assuming a value of the required probability, and weight the doubtful observation accordingly. I then proceed in the ordinary method and determine an approximation to the antecedent probability of the occurrence of a genuine observation giving the value 25 by integrating $\frac{1}{\sqrt{\pi}}\int e^{-t^2}\,dt$ between the limits corresponding to 24.5 and 25.5, since the observations are taken to the nearest unit. This integral is the antecedent probability of an instructive error of the given magnitude, tainted with the incorrectness of the assumption with which I began. Call this integral p. Then $\frac{p}{\frac{1}{10}+p}$ is the resulting required probability. If it agrees with my original assumption, the problem is solved. If it does not agree, I have data for a better assumption according to the well-known method of trial and error. After a few repetitions of the process, as I have found by experiment, an assumption can be made that will be verified by agreement with the result.

In practical problems the antecedent probability of blunders and other uninstructive errors is never known, and is only matter of exceedingly vague conjecture. Perhaps if a very large number of observations were examined, and the proportion of evidently uninstructive errors ascertained, a somewhat intelligent estimate might be made of the proportion of those that exist but are not evident; and data of some little value might be gathered toward a scientific method of weighting. But I have no faith that the result would be any where near worth the labor. At present, the best that a computer can do is to reject entirely, or retain entirely, or assign a simple weight, such as $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{2}{3}$, in sheer desperation, and with the feeling that his judgment is nearly or quite worthless. It would be utter folly to assign weights upon a centesimal scale; and it would also be utter folly to conjecture an antecedent probability and proceed according to the method just set forth.

It is well known that the method of least squares gives very untrustworthy information in regard to the antecedent probability of large instructive errors. In regard to the other antecedent probability required for an intelligent solution of the problem, it gives no information whatever. So far as I can understand Prof. Peirce's method of arriving at a criterion, he takes two probabilities, both functions of probabilities of instructive error, and balances them against each other. This procedure reminds me of what sometimes happens in war, when two detachments of the same army meet in the dark and fire into each other, each supposing the other to belong to the common enemy. Prof. Peirce also seems to me to violate the fundamental principle of the science of probabilities, that probabilities must be independent in order that their product shall equal concurrent probability.

If a computer resorts to the criterion when he feels that his own judgment is worthless, and only then, the criterion is harmless; since it is of no importance whether a decision is made by a worthless judgment or a worthless criterion.

In the discussion that followed, Mr. A. HALL gave a brief account of the literature of the criteria which have been proposed for the rejection of doubtful observations. In addition to the criterion proposed by Prof. Peirce, which had been discussed by Mr. Doolittle, that of Mr. E. J. Stone was mentioned; and also the proofs of a criterion given by Chauvenet and Watson. The advocacy of of Peirce's criterion by Gould, Winlock, Bache, Coffin, and Schott

was noticed, and also its criticism by Airy, Stone, and Glaisher, together with Glaisher's approval of De Morgan's method of treating observations. In conclusion, Mr. HALL said:

The general result of what has been done in this matter appears to be as as follows:

Every one can devise a criterion that suits himself, but it will not please other people.

Now there seems to be a good reason underlying this. The attempt to establish an arbitrary and general criterion for the discussion and rejection of observations is an attempt to eliminate from this work the knowledge and judgment of the investigator. Such an attempt ought to fail, and it certainly will fail at length, no matter by what personal influence it may be supported. It is true that no proof has been given of the principle of the arithmetical mean for a finite number of observations, such as the practical cases that always come before us; but we assume this principle as leading to the most probable result. When we depart from this principle, it must be done, I think, for reasons that are peculiar to each case, and there can be no better guide than the judgment of the investigator. It may be said that if the criteria that have been proposed be carefully managed they will do little harm, since the result of the arithmetical mean will be altered very little; and in fact this is their chief recommendation. But by diminishing the value of the real probable error the criteria give to the observations a fictitious accuracy and a weight they do not deserve.

The paper was also discussed by Messrs. HILL, ELLIOTT, FARQUHAR, WOODWARD, and others, including Mr. JAMES MAIN, a visitor—all agreeing, on essential points, with Mr. Doolittle's view.

Mr. R. S. WOODWARD then discussed

THE SPECIAL TREATMENT OF CERTAIN FORMS OF OBSERVATION-EQUATIONS.

[Abstract.]

In a set of observation-equations whose type is

$$x_0 + (t - t_0)\, y - n = v \text{ with weight } p,$$

in which t_0 is an arbitrary constant, the same for each equation, and in which the residuals, v, are supposed to arise solely from errors in the observed quantities, n, it will be best to make

$$t_0 = \frac{[pt]}{[p]}$$

This value of t_0 makes the co-efficient of y in the first normal equation and the co-efficient of x_0 in the second normal equation, zero, and hence gives directly

$$x_0 = \frac{[pn]}{[p]}$$

$$y = \frac{[p(t - t_0)n]}{[p(t - t_0)^2]}$$

The weight of this value of x_0 is a maximum; *i. e.*, the value of x_0 corresponding to $t_0 = \frac{[pt]}{[p]}$ has a greater weight than the value of x_0 corresponding to any other value of t_0.

The probable error of the function $x_0 + \mu y$ is given by the simple formula.

$$\sqrt{\varepsilon_{x_0}^2 + \mu^2 \varepsilon_y^2}$$

in which ε_{x_0} and ε_y are the probable errors of x_0 and y, respectively.

The investigation shows that, when several standards of length are to be intercompared two and two, in order to obtain the length of some one of them, it will be conducive to accuracy to have the mean temperatures of the several sets of comparisons equal.

Remarks were made upon this communication by Mr. KUMMELL.

Mr. ALEX. S. CHRISTIE made a communication on

CONTACT OF PLANE CURVES.*

[Abstract.]

Let $0 = f(x, y)$, (1), $0 = \varphi(x, y)$, (2), and $y = \psi(x)$, (3) be the equations of plane curves. Transferring the origin to (ξ, η), where $\eta = \psi(\xi)$, writing f, φ for $f(\xi, \eta)$, $\varphi(\xi, \eta)$, respectively, and u_n for $\frac{1}{n!}\frac{\delta^n f}{\delta \xi^n}$, v_n for $\frac{1}{n!}\frac{\delta^n \varphi}{\delta \xi^n}$, we have

from (1), $0 = \sum_0^\infty \left(\frac{y^n}{n!}\frac{\delta^n}{\delta \eta^n} \cdot \sum_0^\infty \left(x^r u_r\right)\right)$, (1′), from (2),

$$0 = \sum_0^\infty \left(\frac{y^n}{n!}\frac{\delta^n}{\delta \eta^n}\right) \cdot \Sigma(x^r v_r), \text{ (2′), and from (3), } y = x\frac{d\eta}{d\xi} + \frac{x^2}{2!}\frac{d^2\eta}{d\xi^2}$$

$$+ \frac{x^3}{3!}\frac{d^3\eta}{d\xi^3} + \&c. \qquad (3')$$

---

* Throughout this paper, δ, for lack of sorts, is put for round d, and denotes partial differentiation.

Writing (3′) in the form $y = xw_1 + x^2w_2 + x^3w_3 + \&c.$ (3″)

and assuming $y^\nu = x^\nu(\nu_0) + x^{\nu+2}(\nu_2) + \&c.$ (4)

Where (ν_0) obviously equals $w_1{}^\nu$, and (ν_1), (ν_2), &c., are functions of ξ, η to be determined, we have, from (4), $\nu y^{\nu-1}\dfrac{dy}{dx} = x^{\nu-1}\nu(\nu_0)$

$+ x^\nu . \nu + 1 . (\nu_1) + x^{\nu+1} . \nu + 2 . (\nu_2) + \&c.$ (5)

from (3″), $\dfrac{dy}{dx} = x^0 . 1w_1 + x^1 . 2w_2 + x^2 . 3w_3 + \&c.$ (6)

from (3″, 5), $\nu\, y^\nu . \dfrac{dy}{dx} = x^\nu((\nu_0)\nu w_1) + x^{\nu+1}((\nu_0)\nu w_2 + (\nu_1) . \nu + 1 . w_1)$

$+ x^{\nu+2}((\nu_0)\nu w_3 + (\nu_1) . \nu + 1 . w_2 + (\nu_2) . \nu + 2 . w_1) + \&c.$

from (4, 6), $\nu y^\nu . \dfrac{dy}{dx} = x^\nu(\nu_0)\nu w_1 + x^{\nu+1}\Big((\nu_0)\nu . 2w_2 + (\nu_1)\nu . 1w_1\Big)$

$+ x^{\nu+2}((\nu_0)\nu . 3w_3 + (\nu_1)\nu . 2w_2 + (\nu_2)\nu . 1w_1) + \&c.$

$\because\ 0 = (\nu_0) . \nu - 0 . w^2 + (\nu_1) . 0 - 1 . w_1$

$0 = (\nu_0) . 2\nu - 0 . w_3 + (\nu_1) . \nu - 1 . w_2 + (\nu_2) . 0 - 2 . w_1$

$0 = (\nu_0) . 3\nu - 0 . w_4 + (\nu_1) . 2\nu - 1 . w_3 + (\nu_2) . \nu - 2 . w_2$

$+ (\nu_3) . 0 - 3 . w_1$

. . . .

$0 = (\nu_0) . m\nu - 0 . w_{m+1} + (\nu_1) . (m-1)\nu - 1 . w_m$

$+ (\nu_2) . (m-2)\nu - 2 . w_{m-1} + (\nu_3) . (m-3)\nu - 3 . w_{m-2}$

$+ \ldots + (\nu_m) . 0 - m . w_1$

Whence we have—

$$(\nu_m) = \frac{(w_1)^{\nu - m}}{m!} \times$$

$$\left| \begin{array}{llllll}
\nu - 0 \,.\, w_2 , & 0 - 1 \,.\, w_1 , & 0 , & \ldots\, 0 & , 0 \\
2\nu - 0 \,.\, w_3 , & \nu - 1 \,.\, w_2 , & 0 - 2 \,.\, w_1 , & \ldots\, 0 & , 0 \\
3\nu - 0 \,.\, w_4 , & 2\nu - 1 \,.\, w_3 , & \nu - 2 \,.\, w_2 , & \ldots\, 0 & , 0 \\
\vdots & \vdots & \vdots & \vdots\ \vdots & \vdots \\
(m-2)\nu - 0 \,.\, w_{m-1} , & (m-3)\nu - 1 \,.\, w_{m-2} , & (m-4)\nu - 2 \,.\, w_{m-3} , & \ldots\, 0 - (m-2) \,.\, w_1 & , 0 \\
(m-1)\nu - 0 \,.\, w_m , & (m-2)\nu - 1 \,.\, w_{m-1} , & (m-3)\nu - 2 \,.\, w_{m-2} , & \ldots\, \nu - (m-2) \,.\, w_2 & , 0 - (m-1) \,.\, w_1 \\
m\nu - 0 \,.\, w_{m+1} , & (m-1)\nu - 1 \,.\, w_m , & (m-2)\nu - 2 \,.\, w_{m-1} , & \ldots\, 2\nu - (m-2) \,.\, w_3 & , \nu - (m-1) \,.\, w_2
\end{array} \right| \quad (7)$$

which determines the coefficients in (4). (*) Putting u_{rs} for $\frac{1}{r!,\ s!,}\frac{\delta^{r+s}f}{\delta\xi^r\delta\eta^s}$, we have $\frac{y^\nu}{\nu!,}\frac{\delta^\nu}{\delta\eta^\nu}\overset{\infty}{\underset{0}{\Sigma}}(x^r u_r) = x^\nu\,((\nu_0)\,u_{0\nu}) + x^{\nu+1}\,((\nu_0)\,u_{1\nu} + (\nu_1)\,u_{0\nu}) + x^{\nu+2}\,((\nu_0)\,u_{2\nu} + (\nu_1)\,u_{1\nu} + (\nu_2)\,u_{0\nu})$ + &c., and this in (1′) gives an equation of the form

$$0 = x^0 A_0 + x^1 A_1 + x^2 A_2 + x^3 A_3 + \&c. \tag{8}$$

viz: $0 = x^0\,[(0_0)\,u_{00}] + x^1\,[(0_0)\,u_{10} + (0_1)\,u_{00} + (1_0)\,u_{01}]$
$+ x^2\,[(0_0)\,u_{20} + (0_1)\,u_{10} + (0_2)\,u_{00} + (1_0)\,u_{11} + (1_1)\,u_{01} + (2_0)\,u_{02}]$
$+ x^3\,[(0_0)\,u_{30} + (0_1)\,u_{20} + (0_2)\,u_{10} + (0_3)\,u_{00} + (1_0)\,u_{21} + (1_1)\,u_{11}$
$+ (1_2)\,u_{01} + (2_0)\,u_{12} + (2_1)\,u_{02} + (3_0)\,u_{03}]$ + &c. (8′)

for the abscissae of points common to (1) and (3). Similarly for the abscissae of points common to (2) and (3) we get an equation of the form

$$0 = x^0 B_0 + x^1 B_1 + x^2 B_2 + x^3 B_3 + \&c. \tag{9}$$

viz: $0 = x^0\,[(0_0)\,v_{00}] + x^1\,[(0_0)\,v_{10} + (0_1)\,v_{00} + (1_0)\,v_{01}]$ + &c. (9′)

Let (2) contain at least p parameters, enabling us to pass (2) through p of the intersections of (1) with (3). When this is done we have the equation $0 = x^0\,(A_0 - B_0) + x^1\,(A_1 - B_1) + x^2\,(A_2 - B_2)$ + &c. (10) true for the p values of x corresponding to the p points common to (1), (2), (3). Let the p common points move to the origin, (10) must have p roots equal zero, that is, $0 = A_0 - B_0$, $0 = A_1 - B_1$, $0 = A_2 - B_2$, . . . $0 = A_{p-1} - B_{p-1}$ (11)

If we suppose (3) the parabolic representative of (1), x in (8) becomes indeterminate, and hence besides $0 = A_0$ we have also $0 = A_1$, $0 = A_2$, &c.

that is, $0 = f$, with

$$\left\{\begin{array}{l} 0 = \frac{\delta f}{\delta\xi} + \frac{d\eta}{d\xi}\frac{\delta f}{\delta\eta} \\ 0 = \frac{1}{2}\frac{\delta^2 f}{\delta\xi^2} + \frac{d\eta}{d\xi}\frac{\delta^2 f}{\delta\xi\delta\eta} + \frac{1}{2}\frac{d^2\eta}{d\xi^2}\frac{\delta f}{\delta\eta} + \frac{1}{2}\left(\frac{d\eta}{d\xi}\right)^2\frac{\delta^2 f}{\delta\eta^2} \\ 0 = \frac{1}{3!}\frac{\delta^3 f}{\delta\xi^3} + \frac{1}{2!}\frac{d\eta}{d\xi}\frac{\delta^3 f}{\delta\xi^2\delta\eta} + \frac{1}{2!}\frac{d^2\eta}{d\xi^2}\frac{\delta^2 f}{\delta\xi\delta\eta} + \frac{1}{3!}\frac{d^3\eta}{d\xi^3}\frac{\delta f}{\delta\eta} \\ \quad + \frac{1}{2!}\left(\frac{d\eta}{d\xi}\right)^2\frac{\delta^3 f}{\delta\xi\delta\eta^2} + \frac{1}{2!}\frac{d\eta}{d\xi}\frac{d^2\eta}{d\xi^2}\frac{\delta^2 f}{\delta\eta^2} + \frac{1}{3!}\left(\frac{d\eta}{d\xi}\right)^3\frac{\delta^3 f}{\delta\eta^3} \\ \quad \&c. \qquad\qquad \&c. \qquad\qquad \&c. \end{array}\right. \tag{12}$$

---

* Putting $x = 1$ in (3″) and (4), we obtain the multinomial theorem in the form $(w_1 + w_2 + w_3 + \&c.)^\nu = (\nu_0) + (\nu_1) + (\nu_2) + \&c.$

equations fully determining $\frac{d\eta}{d\xi}$, $\frac{d^2\eta}{d\xi^2}$ $\cdot$ $\frac{d^3\eta}{d\xi^3}$, &c., in terms of the partial derivatives of f.

Again, suppose (3) the parabolic representative of (2), then $0 = B_0$, with $0 = B_1$, $0 = B_2$, &c., and consequently by (11) $0 = A_0$, with $0 = A_1$, $0 = A_2$, . . . $0 = A_{p-1}$, or the first $p-1$ of the equations (12) are satisfied indifferently whether the $\frac{d\eta}{d\xi}$, $\frac{d^2\eta}{d\xi^2}$, . . . $\frac{d^{p-1}\eta}{d\xi^{p-1}}$ therein contained be derived from (1) or (2); that is, we have arrived at Lagrange's conditions for contact of the $(p-1)$ order, as a consequence of p - punctual contact; and it follows at once that the distance between two curves in the neighborhood of a p - tuple common point is of the p^{th} order when the distance along the curves from the p - tuple point is of the 1st order.*

NOTE.

The abstracts of communications to the Mathematical Section have each been examined by a special committee, consisting of the Chairman, the Secretary, and a third member appointed by the Chairman. These third members were as follows:

Title.	*Author.*	*Third Member.*
Alignment Curves on any Surface	C. H. KUMMELL.	A. S. CHRISTIE.
The Mass of a Planet from Observations of two Satellites	A. HALL.	W. B. TAYLOR.
Infinites and Infinitesimals	M. H. DOOLITTLE.	G. W. HILL.
Planetary Perturbations of the Moon	G. W. HILL.	E. B. ELLIOTT.
The Law of Error practically tested by Target-Shooting	C. H. KUMMELL.	A. S. CHRISTIE.
Form of Least-Square Computation	H. FARQUHAR.	R. S. WOODWARD.
Rejection of Doubtful Observations	M. H. DOOLITTLE.	W. C. WINLOCK.
Special Treatment of certain forms of Observation-Equations	R. S. WOODWARD.	W. C. WINLOCK.
Contact of Plane Curves	A. S. CHRISTIE.	C. H. KUMMELL.

* This paper will be continued.

CORRIGENDA.

Vol. V, p. 86, line 2.	For "abused" read *absurd.*
" " " 7.	For "east" read *earth.*

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. VII.

Containing the Minutes of the Society and of the Mathematical Section for the year 1884.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON:
1885.

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WASHINGTON, D. C.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

CONSTITUTION, RULES,

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OFFICERS AND MEMBERS,

AND REPORTS OF

SECRETARIES AND TREASURER.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

Article I. The name of this Society shall be The Philosophical Society of Washington.

Article II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

Article. III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

Article IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

Article V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

Article VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

---

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who, having complied with Section 13 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz, four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. Any paper read before a Section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

12. New members may be proposed in writing by three members of the Society for election by the General Committee; but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

13. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

14. The fiscal year shall terminate with the Annual Meeting.

15. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments. They can, however, resume their membership by giving notice to the President of their wish to do so.

16. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

---

1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. If the author of any paper read before a Section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee to be appointed as the Section may determine.

The report of this committee shall be forwarded to the Publication Committee by the Secretary of the Section, together with any action of the Section taken thereon.

6. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 22, 1883.

President	J. C. WELLING.	
Vice-Presidents	J. S. BILLINGS.	GARRICK MALLERY.
	J. E. HILGARD.	ASAPH HALL.
Treasurer	CLEVELAND ABBE.	
Secretaries	HENRY FARQUHAR.	G. K. GILBERT.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

H. H. BATES.	E. B. ELLIOTT.
W. H. DALL.	ROBERT FLETCHER.
C. E. DUTTON.	WILLIAM HARKNESS.
J. R. EASTMAN.	J. J. KNOX. *

C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* HENRY FARQUHAR. G. K. GILBERT.

On Publications:

G. K. GILBERT, *Chairman.* CLEVELAND ABBE. HENRY FARQUHAR.

S. F BAIRD†

* Mr. Knox resigned May 10, 1884, and the General Committee elected Mr. F. W. Clarke to the vacancy.

† As Secretary of the Smithsonian Institution.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 20, 1884.

President	ASAPH HALL.	
Vice-Presidents	J. S. BILLINGS.	GARRICK MALLERY.
	WILLIAM HARKNESS.	J. E. HILGARD.
Treasurer	ROBERT FLETCHER.	
Secretaries	G. K. GILBERT.	HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

MARCUS BAKER.	H. H. BATES.
F. W. CLARKE.	W. H. DALL.
C. E. DUTTON.	J. R. EASTMAN.
E. B. ELLIOTT.	H. M. PAUL.

C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* G. K. GILBERT. HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman.* ROBERT FLETCHER. HENRY FARQUHAR.
S. F. BAIRD.*

*As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to December 20, 1884.

The names of founders are printed in SMALL CAPITALS.

(*d*) indicates *deceased*.

(*a*) indicates *absent* from the District of Columbia and excused from payment of dues until announcing his return.

(*r*) indicates *resigned*.

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Abbe, Cleveland	Army Signal Office. 2017 I St. N. W	1871, Oct. 29
Abert, Sylvanus Thayer	1724 Penn. Ave. N. W	1875, Jan. 30
Adams, Henry	1607 H St. N. W	1881, Feb. 5
Aldis, Asa Owen	1765 Mass. Ave	1873, Mar. 1
Allen, James	Army Signal Office. 1907 I St. N.W	1882, Feb. 25
Alvord, Benjamin (*d*)		1872, Mar. 23
ANTISELL, THOMAS	Patent Office. 1311 Q St. N. W	1871, Mar. 13
Avery, Robert Stanton	Coast and Geodetic Survey Office. 320 A St. S. E.	1879, Oct. 11
Babcock, Orville Elias (*d*)		1871, June 9
Bailey, Theodorus (*d*)		1873, Mar. 1
BAIRD, SPENCER FULLERTON	Smithsonian Institution. 1445 Mass. Ave. N. W.	1871, Mar. 13
Baker, Frank	326 C St. N. W	1881, May 14
Baker, Marcus	Coast and Geodetic Survey Office. 1205 Rhode Island Ave.	1876, Mar. 11
Bancroft, George	1623 H St. N. W., or Newport, R. I	1875, Jan. 16
Barnard, William Stebbins	Agricultural Department. 917 N. Y. Ave. N. W., or Canton, Ill.	1884, Mar. 1
BARNES, JOSEPH K. (*d*)		1871, Mar. 13
Bates, Henry Hobart	Patent Office. The Portland	1871, Nov. 4
Bean, Tarleton Hoffman	National Museum. 1411 R. I. Ave	1884, Apr. 26
Beardslee, Lester Anthony (*a*)	Captain U. S. N., Navy Department	1875, Feb. 27
Bell, Alexander Graham	Scott Circle. 1500 R. I. Ave	1879, Mar. 29
Bell, Chichester Alexander	1221 Conn. Ave	1881, Oct. 8
BENÉT, STEPHEN VINCENT	Ordnance Office, War Department. 1717 I St. N. W.	1871, Mar. 13
Bessels, Emil	Smithsonian Institution. 1444 N St. N. W.	1875, Jan. 16.
BILLINGS, JOHN SHAW	Surg. Genl's Office, U. S. A. 3027 N St. N. W.	1871, Mar. 13
Birney, William	456 Louisiana Ave. 1901 Harewood Ave., Le Droit Park.	1879, Mar. 29
Birnie, Rogers (*a*)	Cold Spring, Putnam Co., N. Y	1876, Mar. 11
Blair, Henry Wayne (*d*)		1884, Feb. 2
Bodfish, Sumner Homer	Geological Survey. 605 F St. N. W	1883, Mar. 24
Boutelle, Charles Otis	Coast and Geodetic Survey Office. 1513 20th St. N. W.	1884, Feb. 16
Bowles, Francis Tiffany	1823 Jefferson Place	1884, Mar. 29
Brown, Stimson Joseph	Naval Observatory. 2133 K St. N.W	1884, Apr. 12
Browne, John Mills	Medical Director, U. S. N. The Portland.	1883, Nov. 24
Burchard, Horatio Chapin	Director of the Mint. Riggs House	1879, May 10

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Burgess, Edward Sandford	High School. 810 12th St. N. W	1883, Mar. 24
Burnett, Swan Moses	1215 I St. N. W	1879, Mar. 29
Busey, Samuel Clagett	1525 I St. N. W	1874, Jan. 17
CAPRON, HORACE	The Portland	1871, Mar. 13
Case, Augustus Ludlow (a)	Bristol, R. I	1872, Nov. 16
CASEY, THOMAS LINCOLN	Col. Corps of Engineers. 1419 K St. N. W.	1871, Mar. 13
Caziarc, Louis Vasmer (a)	War Department	1882, Feb. 25
CHASE, SALMON PORTLAND (d)		1871, Mar. 13
Chamberlin, Thomas Crowder	Geological Survey	1883, Mar. 24
Chickering, John White, Jr	Deaf Mute College, Kendall Green	1874, Apr. 11
Christie, Alexander Smyth	Coast and Geodetic Survey Office. 628 Mass. Ave. N. W.	1880, Dec. 4
Clapp, William Henry (a)	Ft. Davis, Tex. 1416 Corcoran St. Washington.	1882, Feb. 25
Clark, Edward	Architect's Office, Capitol. 417 4th St. N. W.	1877, Feb. 24
Clark, Ezra Westcote	Revenue Marine Bureau, Treasury Department. Woodley Road.	1882, Mar. 25
Clarke, Frank Wigglesworth	Geological Survey. 1425 Q St. N. W	1874, Apr. 11
COFFIN, JOHN HUNTINGTON CRANE	1901 I St. N. W	1871, Mar. 13
Collins, Frederick (d)		1879, Oct. 21
Comstock, John Henry (a)	Cornell University, Ithaca, N. Y	1880, Feb. 14
Coues, Elliott	Smithsonian Inst. 1726 N. St. N. W	1874, Jan. 17
CRAIG, BENJAMIN FANEUIL (d)		1871, Mar. 13
Craig, Robert	Army Signal Office. 1008 I St. N. W	1873, Jan. 4
Craig, Thomas (a)	Johns Hopkins Univ., Baltimore, Md.	1879, Nov. 22
CRANE, CHARLES HENRY (d)		1871, Mar. 13
Curtis, George Edward	Army Signal Office. 1416 Corcoran St.	1884, Jan. 5
Curtis, Josiah (d)		1874, Mar. 28
Cutts, Richard Dominicus (d)		1871, Apr. 29
DALL, WILLIAM HEALEY	Care Smithsonian Institution. 1119 12th St. N. W.	1871, Mar. 13
Davis, Charles Henry (d)		1874, Jan. 17
Davis, Charles Henry	Navy Department. 1705 Rhode Island Ave. N. W.	1880, June 19
Dean, Richard Crain (a)	Naval Hospital, New York	1872, Apr. 23
De Caindry, William Augustin	Commissary General's Office. 924 19th St. N. W.	1881, Apr. 30
De Land, Theodore Louis	Treasury Dept. 126 7th St. N. E	1880, Dec. 18
Dewey, Frederick Perkins	National Museum. 1007 G St. N. W	1884, Apr. 25
Dewey, George (r)		1879, Feb. 15
Diller, Joseph Silas	Geological Survey	1884, Mar. 1
Doolittle, Myrick Hascall	Coast and Geodetic Survey Office. 1925 I St. N. W.	1876, Feb. 12
Dorr, Frederic William (d)		1874, Jan. 17
Dunwoody, Henry Harrison Chase (a)	Army Signal Office. 3012 Dumbarton St., Georgetown.	1873, Dec. 20
Dutton, Clarence Edward	Geological Survey	1872, Jan. 27
DYER, ALEXANDER B. (d)		1871, Mar. 13
Earll, Robert Edward	National Museum	1884, Apr. 26
Eastman, John Robie	Naval Observatory. 1823 I St. N. W	1871, May 27
EATON, AMOS BEEBE (d)		1871, Mar. 13
Eaton, John	Bureau of Education, Interior Dept. 712 East Capitol St.	1874, May 8
Eimbeck, William	Coast and Geodetic Survey Office	1884, Feb. 2
Eldredge, Stewart (a)	Yokohama, Japan.	1871, June 9
ELLIOT, GEORGE HENRY (r)		1871, Mar. 13
ELLIOTT, EZEKIEL BROWN	Government Actuary, Treasury Department. 1210 G St. N. W.	1871, Mar. 13
Emmons, Samuel Franklin	Geological Survey. 23 Lafayette Place.	1883, Apr. 7
Endlich, Frederic Miller (a)	Smithsonian Institution. Lake Valley, New Mexico.	1873, Mar. 1
Ewing, Charles (d)		1874, Jan. 17
Ewing, Hugh (a)	Lancaster, Ohio	1874, Jan. 17
Farquhar, Edward	Patent Office Library. 1915 H St. N.W.	1876, Feb. 12

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Farquhar, Henry	Coast and Geodetic Survey Office. Brooks Station, D. C.	1881, May 14
Ferrel, William	Army Signal Office. 471 C St. N. W.	1872, Nov. 16
Fletcher, Robert	Surgeon Genl's Office, U. S. A. 1326 L St. N. W.	1873, Apr. 10
Flint, Albert Stowell	Naval Observatory. 1450 Chapin St., College Hill.	1882, Mar. 25
Flint, James Milton	Navy Dept. U. S. S. Albatross	1881, Mar. 19
Foote, Elisha (*d*)		1871, Mar. 13
Foster, John Gray (*d*)		1873, Jan. 18
French, Henry Flagg (*r*)		1882, Mar. 25
Fristoe, Edward T.	1434 N St. N. W.	1873, Mar. 29
Gale, Leonard Dunnell (*d*)		1874, Jan. 17
Gallaudet, Edward Miner	Deaf Mute College, Kendall Green	1875, Feb. 27
Gannett, Henry	Geological Survey. 1881 Harewood Ave., Le Droit Park.	1874, Apr. 11
Gardiner, James Terry (*a*)	State Survey, Albany, N. Y.	1874, Jan. 17
Garnett, Alexander Young P. (*r*)		1878, Mar. 16
Gihon, Albert Leary	Naval Hospital, 2019 Hillyer Place N. W.	1880, Dec. 18
Gilbert, Grove Karl	Geological Survey. 1424 Corcoran St.	1873, June 7
Gill, Theodore Nicholas	Smithsonian Institution	1871, Mar. 13
Godding, William Whitney	Government Asylum for the Insane.	1879, Mar. 29
Goode, George Brown	National Museum. 1620 Mass. Ave. N. W.	1874, Jan. 31
Goodfellow, Edward	Coast and Geodetic Survey Office	1875, Dec. 18
Goodfellow, Henry (*r*)		1871, Nov. 4
Gore, James Howard	Columbian Univ. 1305 Q St. N. W.	1880, Mar. 14
Graves, Edward Oziel (*a*)	Asst. Treasurer U. S.	1874, Apr. 11
Graves, Walter Hayden (*a*)	Denver, Colorado	1878, May 25
Greely, Adolphus Washington	Army Signal Office. 1909 I St.	1880, June 19
Green, Bernard Richardson	1738 N St. N. W.	1879, Feb. 15
Green, Francis Mathews (*a*)	Navy Department	1875, Nov. 9
Greene, Benjamin Franklin (*a*)	West Lebanon, N. H.	1871, Mar. 13
Greene, Francis Vinton	District Commissioners' Office, 1915 G St. N. W.	1875, Apr. 10
Gregory, John Milton	15 Grant Place	1884, Mar. 29
Gunnell, Francis M.	Surgeon General, U. S. N. 600 20th St. N. W.	1879, Feb. 1
Hains, Peter Conover	1824 Jefferson Place	1879, Feb. 15
Hall, Asaph	Naval Observatory. 2715 N St. N. W.	1871, Mar. 13
Hall, Asaph, jr.	Naval Observatory. 2715 N St. N. W.	1880, Dec. 20
Hanscom, Isaiah (*d*)		1873, Dec. 20
Harkness, William	Naval Observatory. 1415 G St. N. W.	1871, Mar. 13
Hassler, Ferdinand Augustus (*a*)	Santa Ana, Los Angeles Co., Cal.	1880, May 8
Hayden, Ferdinand Vandeveer (*a*)	Geological Survey. 1803 Arch St., Philadelphia, Penn.	1871, Mar. 13
Hazen, Henry Allen	P. O. Box No. 427. 1416 Corcoran St.	1882, Mar. 25
Hazen, William Babcock	Army Signal Office. 1601 K. St. N. W.	1881, Feb. 5
Heap, David Porter	Light House Board, Treasury Department. 1618 Rhode Island Ave.	1884, Mar. 15
Henry, Joseph (*d*)		1871, Mar. 13
Henshaw, Henry Wetherbee	Bureau of Ethnology, P. O. Box 585	1874, Apr. 11
Hilgard, Julius Erasmus	Coast and Geodetic Survey Office. 1709 Rhode Island Ave. N. W.	1871, Mar. 13
Hill, George William	Nautical Almanac Office. 314 Ind. Ave. N. W.	1879, Feb. 1
Hitchcock, Romyn	P. O. Box 630	1884, Apr. 26
Holden, Edward Singleton (*a*)	Madison, Wisconsin	1873, June 21
Holmes, William Henry	Geological Survey. 1100 O St. N. W.	1879, Mar. 29
Hough, Franklin Benjamin (*a*)	Agricultural Dept. Lowville, N. Y.	1879, Mar. 29
Howell, Edwin Eugene (*a*)	Rochester N. Y.	1874, Jan. 31
Humphreys, Andrew Atkinson (*d*)		1871, Mar. 13
Jackson, Henry Arundel Lambe (*a*)	War Department	1875, Jan. 30
James, Owen (*a*)	Scranton, Pa.	1880, Jan. 3
Jeffers, William Nicolson (*r*)		1877, Feb. 24
Jenkins, Thornton Alexander	2115 Penn. Ave. N. W.	1871, Mar. 13

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Johnson, Arnold Burges	Light House Board, Treasury Dept. 501 Maple Ave., Le Droit Park.	1878, Jan. 19
Johnson, Joseph Taber	926 17th St. N. W	1879, Mar. 29
Johnson, Willard Drake	Geological Survey. 501 Maple Ave., Le Droit Park.	1884, Feb. 16
Johnston, William Waring	1603 K St. N. W	1873, Jan. 21
Kampf, Ferdinand (*d*)		1875, Dec. 18
Kauffmann, Samuel Hays	1000 M St. N. W	1884, Feb. 16
Keith, Reuel	2219 I St	1871, Oct. 29
Kerr, Mark Brickell	Geological Survey. 812 21st St. N. W	1884, Feb. 16
Kerr, Washington Caruthers (*a*)	Raleigh, N. C	1883, Apr. 7
Kidder, Jerome Henry	Smithsonian Inst. 1816 N St., N. W	1880, May 8
Kilbourne, Charles Evans (*a*)	War Department	1880, June 19
King, Albert Freeman Africanus	726 13th St. N. W	1875, Jan. 16
King, Clarence (*r*)		1879, May 10
Knox, John Jay (*a*)	Nat. Bk. Republic, New York City	1874, May 8
Kummell, Charles Hugo	Coast and Geodetic Survey Office. 608 Q St. N. W.	1882, Mar. 25
LANE, JONATHAN HOMER (*d*)		1871, Mar. 13
Lawrence, William	First Comptroller's Office, Treasury Department. 1344 Vermont Ave.	1884, Feb. 16
Lawver, Winfield Peter	Mint Bureau, Treasury Department. 1912 I St. N. W.	1881, Feb. 19
Lee, William	2111 Penn. Ave. N. W	1874, Jan. 17
Lefavour, Edward Brown	Coast and Geodetic Survey Office. 905 O St. N. W.	1882, Dec. 16
Lincoln, Nathan Smith	1514 H St. N W	1871, May 27
Lockwood, Henry H. (*r*)		1871, Oct. 29
Loomis, Eben Jenks	Nautical Almanac Office. 1413 College Hill Terrace N. W.	1880, Feb. 14
Lull, Edward Phelps (*a*)	74 Cedar St., Roxbury, Mass	1875, Dec. 4
Lyford, Stephen Carr (*r*)		1873, Jan. 18
MacCauley, Henry Clay (*a*)	P. O. Box 953, Minneapolis, Minn	1880, Jan. 3
McGee, W J	Geological Survey. 1424 Corcoran St.	1883, Nov. 10
McGuire, Frederick Bauders	1306 F St. N. W. 614 E. St. N. W	1879, Feb. 15
Mack, Oscar A. (*d*)		1872, Jan. 27
McMurtrie, William (*a*)	Champaign, Ill	1876, Feb. 26
Maher, James Arran	Geological Survey. 21 E St. N. W	1884, Feb. 16
Mallery, Garrick	Bureau of Ethnology, P. O. Box 585. 1323 N St. N. W.	1875, Jan. 30
Marcou, John Belknap	Geological Survey	1884, Mar. 29
Marvin, Joseph Badger (*a*)	Internal Revenue Bureau	1878, May 25
Marvin, Archibald Robertson (*d*)		1874, Jan. 31
Mason, Otis Tufton	National Museum. 1305 Q St. N. W	1875, Jan. 30
Matthews, Washington	Surgeon General's Office, U. S. A	1884, June 7
MEEK, FIELDING BRADFORD (*d*)		1871, Mar. 13
Meigs, Montgomery (*a*)	U. S. Engineer Office, Keokuk, Iowa	1877, Mar. 24
MEIGS, MONTGOMERY CUNNINGHAM	1239 Vermont Ave. N. W	1871, Mar. 13
Merrill, George Perkins	National Museum	1884, Apr. 26
Milner, James William (*d*)		1874, Jan. 31
Morgan, Ethelbert Carroll	918 E St. N. W.	1883, Oct. 13
Morris, Martin Ferdinand (*r*)		1877, Feb. 24
Murdoch, John	Smithsonian Institution. 1441 Chapin St., College Hill.	1884, Apr. 26
Mussey, Reuben Delavan	P. O. Box 618. 508 5th St. N. W	1881, Dec. 3
MYER, ALBERT J. (*d*)		1871, Mar. 13
Myers, William (*a*)	War Department	1871, June 23
NEWCOMB, SIMON	Navy Department	1871, Mar. 13
Nichols, Charles Henry (*a*)	Bloomingdale, N. Y	1872, May 4
NICHOLSON, WALTER LAMB	1322 I St. N. W	1871, Mar. 13
Nordhoff, Charles	1731 K St	1879, May 10
Norris, Basil	1829 G St. N. W	1884, Mar. 1
Ogden, Herbert Gouverneur	Coast and Geodetic Survey Office. 1324 19th St. N. W.	1784, Feb. 2
Osborne, John Walter	212 Delaware Ave. N. E	1878, Dec. 7

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
OTIS, GEORGE ALEXANDER (*d*)		1871, Mar. 13
PARKE, JOHN GRUBB	Engineer Bureau, War Department. 16 Lafayette Square.	1871, Mar. 13
PARKER, PETER	2 Lafayette Square	1871, Mar. 13
Parry, Charles Christopher (*a*)	Davenport, Iowa	1871, May 13
Patterson, Carlile Pollock (*d*)		1871, Nov. 17
Paul, Henry Martyn	Naval Observatory. 109 1st St. N. E.	1877, May 19
Peale, Albert Charles	Geological Survey. 1010 Mass. Ave. N. W.	1874, Apr. 11
PEALE, TITIAN RAMSAY (*a*)	Philadelphia, Penn	1871, Mar. 13
PEIRCE, BENJAMIN (*d*)		1871, Mar. 13
Peirce, Charles Sanders (*a*)	Coast and Geodetic Survey Office	1873, Mar. 1
Pilling, James Constantine	Geological Survey. 918 M St. N. W.	1881, Feb. 19
Poe, Orlando Metcalfe (*a*)	34 Congress St. West, Detroit, Mich	1873, Oct. 4
Poindexter, William Mundy	701 15th St. N. W. 806 17th St. N. W.	1884, Dec. 20
Pope, Benjamin Franklin	Surgeon General's Office, U. S. A. 1309 20th St. N. W.	1882, Dec. 16
Porter, David Dixon (*r*)		1874, Apr. 11
Powell, John Wesley	Geological Survey. 910 M St. N. W.	1874, Jan. 17
Prentiss, Daniel Webster	1224 9th St. N. W.	1880, Jan. 3
Pritchett, Henry Smith (*a*)	Washington University, St. Louis, Mo.	1879, Mar. 29
Rathbone, Henry Reed (*a*)		1874, Jan. 17
Rathbun, Richard	Smithsonian Institution 1622 Mass. Ave. N. W.	1882, Oct. 7
Ray, Patrick Henry	Army Signal Office	1884, Jan. 5
Renshawe, John Henry	Geological Survey. 1221 O St. N. W.	1883, Feb. 24
Richey, Stephen Olin	732 17th St.	1882, Oct. 7
Ricksecker, Eugene	Geological Survey. 1505 Q St. N. W.	1884, Feb. 16
Ridgway, Robert (*a*)	Smithsonian Inst. 1214 Va. Av. S. W.	1874, Jan. 31
Riley, Charles Valentine	Agricultural Department. 1700 13th St. N. W.	1878, Nov. 9
Riley, John Campbell (*d*)		1877, May 19
Ritter, William Francis McKnight	Nautical Almanac Office. 16 Grant Place.	1879, Oct. 21
Robinson, Thomas	Howard University. 6th St. N. W., cor. Lincoln.	1884, Jan. 19
Rodgers, Christopher Raymond Perry (*a*).	1723 I St. N. W.	1872, Mar. 9
Rodgers, John (*d*)		1872, Nov. 16
Rogers, Joseph Addison (*a*)	Naval Observatory	1872, Mar. 9
Russell, Israel Cook	Geological Survey. 1424 Corcoran St.	1882, Mar. 25
Russell, Thomas	Army Signal Office. 1116 M. St. N. W.	1883, Feb. 10
Salmon, Daniel Elmer	Agricultural Dept. 1006 N St. N. W.	1883, Nov. 24
Sampson, William Thomas (*a*)	Torpedo Station, Newport, R. I.	1883, Mar. 24
SANDS, BENJAMIN FRANKLIN (*d*)		1871, Mar. 13
Saville, James Hamilton	342 D St. N. W. 1315 M St. N. W.	1871, Apr. 29
SCHAEFFER, GEORGE CHRISTIAN (*d*)		1871, Mar. 13
SCHOTT, CHARLES ANTHONY	Coast and Geodetic Survey Office. 212 1st St. S. E.	1871, Mar. 13
Searle, Henry Robinson (*d*)		1877, Dec. 21
Seymour, George Dudley (*r*)		1881, Dec. 3
Shellabarger, Samuel	Room 23 Corcoran Building. 812 17th St. N. W.	1875, Apr. 10
Sherman, John	1319 K St. N. W.	1874, Jan. 17
SHERMAN, WILLIAM TECUMSEH (*r*)		1871, Mar. 13
Shufeldt, Robert Wilson (*a*)	Surgeon Genl's Office, U. S. A., or Box 144 Smithsonian Institution.	1881, Nov. 5
Sicard, Montgomery (*a*)	Ordnance Bureau, Navy Department.	1877, Feb. 24
Sigsbee, Charles Dwight	Naval Academy, Annapolis, Md.	1879, Mar. 1
Skinner, John Oscar	1529 O St. N. W.	1883, Mar. 24
Smiley, Charles Wesley	U. S. Fish Commission, 1443 Mass. Ave. 943 Mass. Ave.	1882, Oct. 7
Smith, David	1330 Corcoran St.	1876, Dec. 2
Smith, Edwin	Coast and Geodetic Survey Office. 2024 Hillyer Place.	1880, Oct. 23
Spofford, Ainsworth Rand	Library of Congress. 1621 Mass. Ave. N. W.	1872, Jan. 27

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Stearns, John (*a*)	Boston, Mass	1874, Mar. 28
Stearns, Robert Edwards Carter	Smithsonian Institution. 1226 Mass. Ave. N. W.	1884, Nov. 22
Stone, Ormond (*a*)	Leander McCormick Observatory, University of Virginia.	1874, Mar. 28
Taylor, Frederick William (*a*)	Smithsonian Institution. Lake Valley, New Mex.	1881, Feb. 19
TAYLOR, WILLIAM BOWER	Smithsonian Inst. 306 C St. N. W	1871, Mar. 13
Thompson, Almon Harris	Geological Survey	1875, Apr. 10
Thompson, Gilbert	Geological Survey. 1448 Q St. N. W	1884, Feb. 16
Tilden, William Calvin (*a*)	New York City	1871, Apr. 29
Todd, David Peck (*a*)	Lawrence Observ., Amherst, Mass	1878, Nov. 23
Toner, Joseph Meredith	615 Louisiana Ave	1873, June 7
True, Frederick William	National Museum	1882, Oct. 7
Twining, William J. (*d*)		1878, Nov. 23
Upton, Jacob Kendrick (*r*)		1878, Feb. 2
Upton, William Wirt	2d Comptroller's Office, Treasury Dept. 1746 M St. N. W.	1882, Mar. 25
Upton, Winslow (*a*)	Brown University, Providence, R. I	1880, Dec. 4
Vasey, George (*r*)		1875, June 5
Walcott, Charles Doolittle	Geological Survey, Nat. Museum	1883, Oct. 13
Waldo, Frank (*a*)	Army Signal Office. Ft. Myer, Va	1881, Dec. 3
Walker, Francis Amasa (*a*)	Mass. Inst. of Technology, Boston, Mass.	1872, Jan. 27
Walling, Henry Francis (*a*)	Geological Survey, Cambridge, Mass	1883, Feb. 24
Ward, Lester Frank	Geological Survey. 1464 R. I. Ave. N. W.	1876, Nov. 18
Webster, Albert Lowry (*a*)	West New Brighton, Staten Island, N. Y.	1882, Mar. 25
Welling, James Clarke	1302 Connecticut Ave	1872, Nov. 16
Wheeler, George M. (*a*)	Engineer Bureau, War Department	1873, June 7
WHEELER, JUNIUS B. (*a*)	Lenoir, N. C	1871, Mar. 13
White, Charles Abiathar	Geological Survey. Le Droit Park	1876, Dec. 16
White, Charles Henry	1744 G St. N. W	1884, Mar. 1
White, Zebulon Lewis (*a*)	Providence, Rhode Island	1880, June 19
Williams, Albert, Jr	Geological Survey. 23 Lafayette Square.	1883, Feb. 24
Wilson, Allen D. (*a*)		1874, Apr. 11
Wilson, James Ormond	Franklin School Building. 1439 Mass. Ave. N. W.	1873, Mar. 1
Winlock, William Crawford	Naval Observatory. 723 20th St. N.W	1880, Dec. 4
Wolcott, Christopher Columbus (*r*)		1875, Feb. 27
Wood, Joseph (*a*)	Supt. Motive Power, Penn. Co., Fort Wayne, Ind.	1875, Jan. 16
Wood, William Maxwell (*a*)	Navy Department	1871, Dec. 2
Woodruff, Thomas Maher	Army Signal Office. 2020 Hillyer Place.	1884, Apr. 12
WOODWARD, JOSEPH JANVIER (*d*)		1871, Mar. 13
Woodward, Robert Simpson	Geological Survey, 1125 17th St. N. W.	1883, Nov. 24
Woodworth, John Maynard (*d*)		1874, Jan. 31
Yarnall, Mordecai (*d*)		1871, Apr. 29
Yarrow, Harry Crécy	814 17th St. N. W	1874, Jan. 31
Yeates, William Smith	Smithsonian Institution. 401 G St. N. W.	1884, Apr. 29
Zumbrock, Anton	Coast and Geodetic Survey Office. 455 C St. N. W.	1875, Jan. 30

Number of *founders*	44	
" members *deceased*		41
" " *absent*		62
" " *resigned*		16
" " *active*		173
Total number enrolled		292

CALENDAR FOR THE USE OF THE PHILOSOPHICAL SOCIETY,

Showing the alternate SATURDAYS for holding Meetings during the several "Seasons" from 1884–'85 to 1907–'08, inclusive.

PREPARED BY MR. E. B. ELLIOTT.

Submitted to the General Committee June 7, 1884, and ordered published.

Years.	October.	November.	December.	Years.	January.	February.	March.	April.	May.	June.
1884	11, 25	8, 22	6, 20	1885	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1885	10, 24	7, 21	5, 19	1886	2, 16, 30	13, 27	13, 27	10, 24	8, 22	5, 19
1886	9, 23	6, 20	4, 18	1887	15, 29	12. 26	12, 26	9, 23	7, 21	4, 18
1887	15, 29	12, 26	10, 24	1888	7, 21	4, 18	3. 17, 31	14, 28	12, 26	9, 23
1888	13, 27	10, 24	8, 22	1889	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1889	12, 26	9, 23	7, 21	1890	4, 18	1, 15	1, 15, 29	12, 26	10, 24	7, 21
1890	11, 25	8, 22	6, 20	1891	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1891	10, 24	7, 21	5, 19	1892	2, 16, 30	13, 27	12, 26	9, 23	7, 21	4, 18
1892	15, 29	12, 26	10, 24	1893	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1893	14, 28	11, 25	9, 23	1894	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1894	13, 27	10, 24	8, 22	1895	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1895	12, 26	9, 23	7, 21	1896	4, 18	1, 15, 29	14, 28	11, 25	9, 23	6, 20
1896	10, 24	7, 21	5, 19	1897	2, 16, 30	13, 27	13, 27	10, 24	8, 22	5, 19
1897	9, 23	6, 20	4, 18	1898	15, 29	12, 26	12, 26	9, 23	7, 21	4, 18
1898	15, 29	12, 26	10, 24	1899	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1899	14, 28	11, 25	9, 23	1900	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1900	13, 27	10, 24	8, 22	1901	5, 19	2, 16	2, 16, 30	13. 27	11, 25	8, 22
1901	12, 26	9, 23	7, 21	1902	4, 18	1, 15	1, 15, 29	12, 26	10, 24	7, 21
1902	11, 25	8, 22	6, 20	1903	3, 17. 31	14, 28	14, 28	11, 25	9, 23	6, 20
1903	10, 24	7, 21	5, 19	1904	2, 16, 30	13, 27	12, 26	9, 23	7, 21	4, 18
1904	15, 29	12, 26	10, 24	1905	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1905	14, 28	11, 25	9, 23	1906	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1906	13, 27	10, 24	8, 22	1907	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1907	12, 26	9, 23	7, 21	1908	4, 18	1, 15, 29	14, 28	11, 25	9, 23	6, 20

ANNUAL REPORT OF THE SECRETARIES.

WASHINGTON CITY, *December* 20, 1884.

To the Philosophical Society of Washington:

We have the honor to present the following statistical data for 1884.

At the beginning of the year the number of active members was	151
This number has been increased by the addition of 35 new members and by the return of 5 absent members. It has been diminished by the departure of 13 members and by the death of 5. There have been no resignations. The net increase of active members has thus been . .	22
And the active membership is now	173

The roll of new members is:

W. S. BARNARD.
T. H. BEAN.
H. W. BLAIR.
C. O. BOUTELLE.
F. T. BOWLES.
S. J. BROWN.
G. E. CURTIS.
F. P. DEWEY.
J. S. DILLER.
R. E. EARLL.
WILLIAM EIMBECK.
ASAPH HALL, Jr.
J. M. GREGORY.
D. P. HEAP.
ROMYN HITCHCOCK.
W. D. JOHNSON.
S. H. KAUFFMANN.
M. B. KERR.
WILLIAM LAWRENCE.
J. A. MAHER.
J. B. MARCOU.
WASHINGTON MATTHEWS.
G. P. MERRILL.
JOHN MURDOCH.
BASIL NORRIS.
H. G. OGDEN.
P. H. RAY.
W. M. POINDEXTER.
EUGENE RICKSECKER.
THOMAS ROBINSON.
R. E. C. STEARNS.
GILBERT THOMPSON.
C. H. WHITE.
T. M. WOODRUFF.
W. S. YEATES.

The names of deceased members are:

BENJAMIN ALVORD. O. E. BABCOCK. H. W. BLAIR. CHARLES EWING. J. J. WOODWARD.

There have been 15 general meetings for the presentation and discussion of papers (not including the public meeting of Dec. 6); the average attendance has been 42. There have been six meetings of the Mathematical Section; average attendance 15.

In the general meeting 32 communications have been presented; in the mathematical section 11. Altogether 43 communications have been made by 32 members and one guest. The number of members who have participated in the discussions is 38. The total number who have contributed to the scientific proceedings is 50, or 29 per cent. of the present active membership.

Very Respectfully,
G. K. GILBERT,
H. FARQUHAR,
Secretaries.

ANNUAL REPORT OF THE TREASURER.

WASHINGTON CITY, *December* 31, 1884.

To the Philosophical Society of Washington:

I have the honor to present herewith my annual statement as Treasurer for the year ending December 20th, 1884.

The revenue of the Society has amounted to $855.00 and the expenditures have been $671.96, leaving a balance of $183.04 on hand; the details of this account are given in the accompanying table.

The investments of the funds of the Society have not changed and consist, therefore, of $1,000 in a U. S. Bond at 4½ per cent. and $1,500 in U. S. Bonds at 4 per cent.

The receipts during the past year may be classified as follows:

Interest on invested fund		$95
5 Dues for 1882,	$25	
16 " " 1883,	80	
126 " " 1884,	630	
2 " " 1885,	10	
149	745	

The dues remaining unpaid are about as follows:

For 1882,	3	$15
" 1883,	10	50
" 1884,	47	235
	60	300

Early in February 500 copies of Volume VI of the Bulletin were received from the printer, and 148 copies have been distributed to active members, also 67 copies have been sent to domestic and 73 to foreign recipients; occasional copies of other volumes have also been sent to complete broken sets. The stock of publications now on hand is about as follows:

Bulletin, Volume I.	91	copies.
" " II.	82	"
" " III.	199	"
" " IV.	184	"
" " V.	201	"
" " VI.	215	"
W. B. Taylor, Memoir of Joseph Henry, 1st Ed.	64	copies.
" " " 2d Ed.	30	"
J. C. Welling, Address on life of Joseph Henry	4	"
W. B. Taylor, Address as President	70	"

In return for the distribution of Bulletins the Society has received about seventy-five publications from other organizations or individuals and the Accessions Catalogue of the Library now includes 177 titles.

Very respectfully your obedient servant,

CLEVELAND ABBE, *Treasurer.*

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer, for the year ending Dec. 20, 1884.* CR.

EXPENDITURES.					RECEIPTS.		
Date.	Vou'r.	Check.	To whom paid.	Amount.	From what source.	Amount.	Total.
1884.					Credit by receipts as follows:		
Feb. 1	1	80	H. Farquhar, expenses as Sec'y	$27 90	Balance carried over from Dec. 31, '83.	-------	-------
Feb. 1	2	81	G. K. Gilbert, " "	8 88	(*See over-draft*, $73.51, *below.*)		
Feb. 1	3	82	Judd & Detweiler, printers	39 55	Annual dues received:		
Feb. 6	4	83	Cleveland Abbe, exp. as Treas.	20 75	and deposited February 2, 1884	$245 00	
Feb. 18	5	85	A. H. Gawler, janitor	10 00	" " " 9, "	155 00	
Feb. 14	6	84	Judd & Detweiler, prs. $200 00		" " April 3, "	100 00	
April 2	6	86	" " 150 00		" " June 11, "	20 00	
July 15	6	89	" " 28 00		" " July 15, "	5 00	
Aug. 1	6	90	" " 41 07		" " August 1, "	95 00	
				419 07	" " Dec'mb'r 16, "	90 00	
May 15	7	87	G. K. Gilbert, exp. as Sec'y	5 37			
June 12	8	88	H. Farquhar, " "	17 15		710 00	
Aug. 1	9	91	J. C. Welling, President	50	Cash in hand December 20, 1884	35 00	
Aug. 1	10	92	Judd & Detweiler, printers	31 60	Repayment for special printing,		
Aug. 1	11	93	A. H. Gawler, janitor	7 00	deposited February 2	15 00	
Dec. 6	12	94	G. K. Gilbert, exp. as Sec'y	10 68	Interest of $1,000 at 4½ $45 00		
					" 1,500 at 4 50 00		
			Total	598 45		95 00	
			Balance on hand	183 04			$855 00
					Less over-draft of Dec., 1883	-------	73 51
				781 49			
					Total	-------	781 49

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ANNUAL ADDRESS OF THE PRESIDENT.

ANNUAL ADDRESS OF THE PRESIDENT,

JAMES C. WELLING.

Delivered December 6, 1884.

THE ATOMIC PHILOSOPHY, PHYSICAL AND METAPHYSICAL.

Every nation under the sun has a philosophy of some kind, but the philosophy we profess draws the lines of its historic traditions, if not its "increasing purpose," from the home of our Aryan ancestors in Greece. It was here that the typical forms of our literature were invented, that the art of sculpture was carried to its climax, and that the architecture of the lintel came to a transfiguration in the Theseum and the Parthenon. And as if all these glories were not enough, it is the further good fortune of the Greeks to have at least opened up the great leading problems of human enquiry, in physics, in psychology, and in ethics; and to have so opened them up at the starting point of the world's Torch-race, that the light shed on these questions more than twenty-five centuries ago is still a matter of curious retrospection to this generation of ours on whom the ends of the world are come.

It is to one of the oldest of the formal physical philosophies ever framed by the mind of man for the explanation of the mechanical structure of the Universe that I purpose to call your attention to-night—a theory the most comprehensive in its scope, and, at the same time, the most searching in its subtility, which has been handed down to us by all antiquity—a theory which in its ingenuity represents the synthetic power of the Greek mind at the highest stage of its physical speculation—a theory which the literature of Rome has preserved in the amber of Cicero's philosophical disquisition, and embalmed in the immortal verse of Lucretius—a theory, in fine, which has survived the old dialectic in which it was first conceived, because it has come to a new birth in the forms of modern science. I refer to what is known in history as the Atomic Philosophy of the Greeks.

The fundamental principle of the ancient physical philosophy—its point of departure and its ever re-entering point of return—is found in the famous well-worn maxim of metaphysics, that out of nothing nothing comes, and that what *is* can never be annihilated. It was in the name of this maxim and under the shadow of its authority that the Greek physical philosophers sought to shelter their whole right of free enquiry from the charge of impiety, and if to us the dictum seems the merest truism, it was not so regarded at the dawn of natural philosophy. Sometimes used as a logical club with which to brain a stolid and incurious indifferentism, and sometimes waved as a red flag in the face of polytheistic superstition, it meets us perpetually in all the oldest records of ancient philosophical speculation—in the formal elaborations of Aristotle,* in the lucubrations of Boëthius,† and in the verse of poets as remote from each other in style and creed as Lucretius, the lively Epicurean,‡ and Persius, the sternest of Stoic moralists.§ This maxim stirred the philosophical mind of antiquity to its lowest depth, because it was then the type and symbol of a whole method of philosophizing—a method regarded by many as not a little presumptuous, much as the Copernican theory of the Universe was regarded in the sixteenth century, or much as the Formula of Evolution is regarded to-day outside of scientific circles.

It was because the maxim seemed to so many the challenge of a vain wisdom and of a false philosophy that the early champions of physical philosophy sometimes felt themselves called to vindicate the truth of this truism by an appeal to formal argument. The necessity for such an appeal measures the scientific ineptitude of the average mind at that early age. "If what emerges into sensible perception," argues Epicurus with the utmost gravity, "can be conceived as coming from nothing, then everything might come of anything, and that, too, without any need of germs; and if what disappears from sensible perception was really destroyed into nothing, then all things might perish without anything being left into which

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* Aristotle: *De Generatione et Corruptione*, I, iii, 5, (Didot's ed., vol. 2, p. 437.)

† Boëthius: *De Consolatione Philosophiæ*, Lib. V, Prosa 1.

‡ Lucretius: *De Rerum Natura*, I, 151–227.

§ Persius: *Satira*, iii, 84.

they were resolved." * Such was the rude flint-flake with which, as their only weapon of logic, the early Nimrods of philosophy in Greece defended their right to philosophize in the palæolithic stage of natural enquiry.

As the next step in this metaphysical logic we find a distinction drawn by the ancient Greek philosophers between things as they are in substrate and things as they appear, disappear, and reappear in time—between the noumenal and the phenomenal world, as we would say to-day in the Kantian phraseology. It was the favorite doctrine of the Eleatic school of philosophers that we get a true conception of things only when, abstracting from their individuality, their partitiveness and their changing forms, we find the ultimate root and unity of all being in a simple, indivisible, and unchangeable substrate, which is the true object of knowledge, because it is the true basis of all reality. This concept increased in clearness as it passed through the minds of Xenophanes, Parmenides, and Empedocles, until, in the generalizations of the last-named philosopher, the ultimate substrate of things was resolved into four elementary substances—earth, air, fire, and water; each uncreated and imperishable, each equal in quantity, each composed, within itself, of parts that are qualitatively the same, and each forever incommutable with the others; yet each and all capable of every variety and degree of mixture in the manifold combinations of things as they appear in the sensible world.

On the other hand, it was held by Heraclitus that this fundamental substrate or unity of things is a mere figment of the philosophical imagination, and that it is only as things are conceived to be in perpetual flux that the forms of our knowledge can be brought into correspondence with the forms of actual being. That is, to the doctrine of the unchanging substrate of things Heraclitus opposed the doctrine of the perpetual flux of things.

It remained to effect a synthesis and reconciliation between these opposing views of the Eleatic and Heraclitic philosophies of nature, while at the same time saving the fundamental dogma of all natural philosophizing, that out of nothing nothing comes. Such a basis of pacification was found in the terms of the Atomic Philosophy, in the doctrine that the changing forms, positions, motions, and phases of

* Diog. Laërt.: Lives of the Philosophers, *sub voce* "Epicurus."

things are to be conceived as a perpetual flux, resulting from the changing permutations and combinations of the indestructible atoms composing the eternal substrate of nature. And thus it was that the doctrine of ultimate atoms, incessantly modified in the forms of their combination, but remaining forever the same in substance, became the legitimate deduction and the crowning corollary of the primal eldest maxim of physical philosophy. Aristotle expressly gives this genesis of the Atomic Philosophy of Greece in its reduction by Anaxagoras. After saying that Anaxagoras hypothesized an infinity of atoms, to explain the myriad varieties of nature, because he wished to avoid the reproach of getting something out of nothing, Aristotle adds: "From the fact that contraries are made out of each other, they must needs have previously existed in each other; for if everything that *becomes* must needs come either from something or from nothing, and if this latter alternative is impossible, (about which all who treat of nature are agreed in opinion,) then it only remains to infer that everything which becomes must have come from the things in which it pre-existed, though, on account of the smallness of their bulks, made out of things imperceptible to us." *

The Atomic Philosophy of the Greeks was, therefore, not a mere exhalation of the imagination, but a logical inference from the starting point and major premise of their natural metaphysics. The doctrine of ultimate atoms in nature was, indeed, the necessary complement and reconciliation of the conception that all things are in elemental stir, and that yet in this elemental stir there is no creation of anything out of nothing and no annihilation of anything, but only composition, decomposition, and recomposition.

It need not surprise us, therefore, to find that the doctrine of ultimate atoms in nature is a universal form of thought among thinking men of all the most advanced races in antiquity. Into the hidden historic springs of the Atomic Philosophy, as formulated by the Greeks, it is not here proposed to enquire. Whether its

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* Aristotle: *Naturalis Auscultatio*, I, iv, 2. (Didot's ed., vol. 2, p. 252.) Compare, also, Lucretius, *De Rer. Nat.*, I, 543–545:

—— "*Quoniam* supra docui nil posse creari
De nilo, neque quod genitum est ad nil revocari,
Esse immortali primordia corpore *debent*."

germs were derived from Egypt, or from India, or from Phœnicia, or whether it was an original birth of the Hellenic mind, is a matter of curious historic interest which hardly admits, perhaps, of precise and positive determination, though certain it is that India had an Atomic Philosophy before the Greeks. However possible or probable it may be that the early Greek philosophers borrowed some of their lore under this head, as we know they did under others, from the Egyptian priests; or whatever truth there may be in the tradition, reported by Posidonius,* (Cicero's teacher in philosophy,) that one Moschus, a Phœnician, imparted the doctrine to Pythagoras, it is very certain that the Greek philosophers have made the doctrine their own by the logical development they gave to it, and by the hereditament in it which they have bequeathed to the subsequent generations of men moving along the lines of human progress. It has been more than suspected that the doctrine dates in Greece from the age of Pythagoras, by reason of certain specific ideas, which we can read in the spectrum analysis of the most distant times by the light of modern anthropological science. Certain definite lines of thought are to be found in the psychology of every epoch, and these lines betray the mental constitution of the epoch as surely as the vapors of the elements absorb rays of the same refrangibilities that they radiate. In the days of Pythagoras we discover certain psychical ideas which are seen to have been the natural reflex of the great fundamental dogma out of which the Atomic Philosophy sprang. I refer to the doctrine of metempsychosis and of its correlate, the pre-existence of souls. If it be assumed that the human soul is something generically different from the body, and is not generated by it, then it necessarily follows, according to the maxim *De nihilo nihil fit*, that the soul pre-existed somewhere before the atoms of the body were put together, and from the other branch of the maxim, that it must continue to exist somewhere after the body is dissolved. The doctrine of the transmigration of souls is not, therefore, a mere vagary of the ethnical imagination, but the natural offspring of that form of Pythagorean dualism which distinguished the soul, as not only generically, but genetically distinct from the body. Hence, the

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*Strabo: *Geog.*, Lib. XVI. *Cf.* Sextus Empiricus: *Adversus Mathematicos*, Lib. 9.

doctrine reappears under one form or another in every dualistic conception of matter and mind—now in the purple light of Plato's imperial fancy, and now in the pallid shades of that metaphysical theology which, in the days of Origen and St. Augustine, arrayed the doctors of the Church into opposing schools on the great question of *Traducianism* or *Creationism.*

In whatever way the Atomic Philosophy was begotten in the Greek mind, we know that on its emergence it was subjected to the solvents of philosophical criticism, and underwent a variety of transformations. So soon as we come within the lines of definite history we find a bifurcation of ideas between such typical teachers as Anaxagoras on the one hand, and Leucippus and Democritus on the other. This dissidence of opinions had regard to the nature and the constitution of the ultimate atoms which compose the substance of matter. The doctrine of Anaxagoras was qualitative; the doctrine of Democritus was quantitative. Anaxagoras held that the atoms which compose the physical Universe in its fleeting forms, and at the same time in its enduring substance, are eternally differentiated in kinds, and that it was by the collocation and adhesion of like parts—of bony atoms to make bone, of fleshy atoms to make flesh, of stony atoms to make stones—that the actual varieties of body in the Universe were built up. This is the famous doctrine of *Homœomeria* which fares so ill in the spiritual philosophy of Plato, and which fares no better in the materialistic philosophy of Lucretius. Aristotle tells us that Anaxagoras was driven to adopt this hypothesis in order to relieve the doctrine of atoms at the points where the heaviest stress and tightest pinch seemed to be laid upon it by the dogma *De nihilo nihil fit;* for how else, said Anaxagoras, could we account for the existing varieties of matter unless there be an original and eternal variety in its constituent elements?

Democritus simplified the theory of atoms by giving to it a purely mechanical reduction. Conceiving atoms to be invisible by reason of their smallness, he at the same time conceived them to be indivisible, not as mathematically considered, but as physically considered; and while holding them to be infinite in number and infinite in their shapes, he at the same time held that they differed not at all in inherent quality, but simply in their shapes, sizes, situations, and motions, and that hence it was by the different combination of

atoms differing in these mechanical aspects that all the varieties of bodies and souls were integrated, disintegrated, and reintegrated.* The different impressions produced on the human senses by different bodies, according to their various mechanical constitutions, were regarded by him as purely subjective—the mechanical results of different concussions made on the senses by the different effluxes of things,† and, therefore, no more requiring any qualitative differences to explain the phenomena of sensation than to explain the phenomena of being. Weight and hardness were treated by Democritus as primary qualities of bodies resulting from the greater or less degrees in which their constituent atoms are compacted, as compared with the interstitial voids or vacua. The secondary qualities of bodies are simply the impressions they make on the human senses, depending, as has just been said, on the varying shapes, sizes, and arrangements of the atoms composing all the varieties of material substance.

Anaxagoras had made his theory of atoms a pendant to dualism, conceiving, as he did, that souls, both animal and rational, are eternally pre-existent before the birth, and post-existent after the dissolution, of the bodies in which they temporarily resided.‡ Democritus made the animal soul and the rational soul only two more distinct varieties in the mechanical collocation of atoms varying in shape, size, situation, and motion; and, therefore, he had no place in his theory for the transmigration of souls considered as entities distinct from bodies. Souls and bodies were equally the results of a concourse of atoms obeying in their movements the law of a mechanical necessity. That is, to the dualism which preceded him Democritus opposed a pure and simple monism. Yet between the materialism of Democritus and the dualism of Anaxagoras there is not much to choose; for the misty spiritualism of the latter did not

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*Aristotle: *De Generat. et Corrup.*, I, i, 4, (Didot's ed., vol. 2, p. 432;) also Arist.: *Metaphysics*, VII, ii, 2, (Didot's ed., vol. 2, p. 559,) and the *Nat. Auscul.*, I, v, 1, (Didot's ed., vol. 2, p. 254.)

†Action from a distance, as of the magnet on iron, was also explained by Democritus on the hypothesis of "effluxes." Zeller: *Philosophie der Griechen*, Erster Theil, p. 704.

‡Cudworth: Intellectual System of the Universe, vol. I, chap. 1, (Andover ed,) p. 95.

carry with it any clear conception of personal identity, and hence Lucretius justly argued that the doctrine of a future life, as held by many in his day, was stripped of all significance if the chain of personal consciousness is broken at death.*

And to this fundamental antithesis of ideas lying at the bottom of these two forms of the Greek Atomic Philosophy another antithesis must be added in the Stratonical Hylozoism, which, assuming in matter an atomic structure partly material and partly vital, proceeded to account for the genesis of animated bodies on the superadded assumption of a plastic energy working in nature to the production of every living thing. In a word, Strato's matter, instinct with life, and waiting only for the first chance to be stuck together in the composition of plants and animals, seems to have been the metaphysical anticipation of our modern protoplasm.†

It was in opposition alike to the physics of Anaxagoras, Democritus, and Strato, that Plato reared his splendid fabric of idealism, while Aristotle, for his part, rejected the philosophy of atoms altogether, and installed in its place for centuries the doctrine of Form and Quality, and Substance and Entelechy, whatever that may mean. "If," he says, "there be no other substance beyond the substances existing in nature, then Physics is the first science; but if there be a certain substance which is immovable, then this is before body, and Philosophy is the first science." ‡ That single sentence recapitulates the whole verbal philosophy of the Middle Ages. Plato was so hostile to the hypothesis of Democritus that he never once names that philosopher in all his writings, though it is the Abderite physicist to whom he intends a disparaging allusion when in the *Timæus* he impales on the shafts of his irony "a certain philosopher of an indefinite and ignorant mind." Aristotle names him often enough, either separately or in conjunction with Leucippus, and treats the Atomic Philosophy with respect as an "invention framed to explain the transformation and birth of things—explaining birth and dissolution by the decomposition and recomposition of atoms,

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* Lucret.: *De Rerum Natura*, Lib. iii, 851.

† Cicero aptly defines the antithesis of ideas between Democritus and Strato. See *Academ. Prior.*, Lib. II, xxxviii, 121. Also, *De Nat. Deor.*, Lib. I, xiii, 35.

‡ Arist.: *Met.*, Lib. V, i, 9; *cf.* Lib. X, vii, 9.

and explaining transformations by the arrangement and position of atoms."*

But it is in the physical philosophy of Epicurus, as that philosophy has been expounded and expanded by Lucretius, that we can discover the fullest and clearest exposition of the doctrine of atoms, considered as a key to the structure of the Universe. We here have the doctrine formulated into a theodicy of naturism, a theory of psychology, a cosmogony, and an anthropology. According to Epicurus, in his Lucretian rendering, atoms are minute material particles, indivisible, not by reason of their smallness, but of their solidity which makes them indestructible and unchangeable in their constitution; they have size, weight, and shape, yet are forever invisible to the eye; in shape, some of the atoms are different from the others, but, while the number of the different shapes is finite, the number of atoms of each shape is infinite; every atom must have at least three *cacumina* (γωνίας), that is, infinitesimally small bounding points which are incapable of existing apart from the atom, but must be conceived to coexist with it in order to give definition to it and to enclose its "solid singleness;" some of the atoms are hook-shaped, some only slightly jagged, some smooth, &c.; atoms are in incessant motion, racing through space in all directions under the stress of their weight,† according to the favoring conditions of a vacuum more or less complete, yet so that the sum of their motions results in the supreme repose of gross matter, except when a thing exhibits the motion of translation in space—a form of motion which is molar and not atomic; atoms move besides at an enormous uniform speed, in parallel lines, up and down, so far as there can be any up and down in a universe equally boundless in all directions, and except so far as some of the atoms have originally a shape which makes them capable of slight deflections from parallel straight lines—that *clinamen principiorum* which was invented by Epicurus to explain the phenomena of so-

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*Arist.: *De Generat. et Corrup.*, I, ii, 4 (Didot's ed., vol. 2, p. 434.)

†Epicurus derived the motion of atoms from their weight, which gives movement *in vacuo*. Democritus derived the motion of atoms from an impulse given to them in the beginning. So says Cicero (De Fato, 20, 46), but for the contrary opinion, *cf.* Zeller: *Philos. der Griechen*, Erster Theil, 702, 714.

called voluntary motion in animals and free-will in men, while at the same time explaining how it is that this free-will is eternally encased in the rigid parallel lines of the other atoms. In this way Epicurus supposed himself to have added a useful supplementary hypothesis to the original hypothesis of Democritus, who binding nature fast in fate had not left free the human will, because he had omitted to provide for that third mode of motion in atoms which is required to explain the possibility and genesis of voluntary motion and self determination.

Such is a brief and imperfect exposition of the Atomic Philosophy of the Greeks—a form of physical speculation the most elaborate, the most ingenious, and, to use a Latinism of Dr. Johnson, the most concinnous which has come down to us from all antiquity. The Epicurean physics are as much superior to the Aristotelian and the Stoical physics as the ethics of the Lyceum and of the Porch are superior to the ethics of the Sty; and yet it now remains to be said that in all this operose system of metaphysico-physical atoms there is not an atom of scientific truth, in the modern sense of that word. The whole speculation is a mirage, caused by unequal refractions in the Greek intellect—by the volatility of the Greek fancy passing through a dense, practical ignorance with regard to everything but surface views in nature. Or, to borrow one of Plato's favorite figures, it was a "wind-egg," begotten of metaphysic conceit, and differing from the other "wind-eggs" of that time in the greater symmetry of its shell rather than in the greater fecundation of its contents. It had the form, but not the power of scientific truth. If there be such a thing as atoms they must needs be chemical conceptions, and the very word "chemistry" had not yet come into the Greek language, because the rationale of such a science had not even dawned on the horizon of the Greek intellect by the faintest reflection from below.

Much explanation, which does not explain, has been wasted to account for the incapacity of the Greek mind in physical philosophy. The learned historian of the Inductive Sciences, Dr. William Whewell, ascribes this incapacity to the alleged fact that though this sprightly race had in their possession an abundance of facts, and were acute observers and critics, their ideas "were not distinct and appropriate to the facts."* It would

* William Whewell: History of the Inductive Sciences, vol. I, p. 87.

hardly be possible to frame an explanation more pointless. If there has ever been a hypothesis framed with more "distinct ideas" than that of the Greek atomists, I am not acquainted with it, and it is precisely because it was so "appropriate" to the surface facts of the Greek observation that it was so illusory. It was fitted to these facts with a *concinnitas* that is most admirable from a psychological point of view. It was *invented* to fit them, and its whole *raison d'être* was that it *did* fit them, so far as ideas and words could make it fit. For this purpose it was revised, modified, contracted, enlarged, supplemented, until it seemed to fit every sinuosity of the facts of nature, as far as the facts of nature were open to the apprehension of the Greeks in the 5th century before Christ. The hypothesis was strong just where it seems weak to Dr. Whewell, and it is precisely because it was so ideally strong that it was so physically weak, and it is precisely because it fitted the facts so well that it was a delusion and a snare. Men rested in it with a sense of satisfaction which simulated the rest of a mind turning on the poles of truth. It satisfied the highest cravings of Greek physical enquiry in the then contemporaneous stage of mental evolution in Greece. The Greek mind of that age had not reached a stage of development which required anything more than metaphysical hypotheses for the explanation of physical facts, because it had not reached a stage of evolution which capacitated it to frame hypotheses in physics capable of anything more than metaphysical verification. And hence it was in the ingenuity of a plausible hypothesis, and in the nicety with which it fitted the superficial facts that the subtle and artistic mind of the Greeks found the sole interest and zest which a physical hypothesis had for them or could have. "Ancient logic," says Prof. Jowett, "was always mistaking the truth of the form for the truth of the matter."* The conscious incapacity of the Greeks for physical science was so great that we find the best class of minds among them absolutely revolting at the very idea of such a science. Socrates, for instance, had no patience with it. Plato represents him in the *Phœdo* as at the same time deploring misology—the hatred of formal ideas—and yet, in almost the same breath, confessing himself a misologist in the presence of mechanical conceptions of nature. He liked the doctrines of Anaxagoras well enough, so

*Jowett's Plato, vol. I, p. 376.

far as they moved in mind, but he detested them, to use the words put in his mouth by Plato, so far as they moved in "air, and ether, and water, and such like inconsequences;"* and, detesting them, he falls back upon a purely anthropomorphic conception of the Universe—anthropomorphic because it is avowedly anthropocentric, with Socrates for its centre. The whole passage is a most instructive page in comparative psychology, now that we can read it in the light of modern anthropological science.

It is no part of my present purpose to carry the history of the Atomic Philosophy into Roman speculation. The Romans took all their ideas in mental, moral, and physical philosophy at second-hand from the Greeks.† Strong in the practical arts of war and polity, they were content to be in literature imitators and in philosophy eclectics. Equally inept for the deft metaphysical analysis of the Greeks and for their fine artistic synthesis, the Romans none the less contributed, on the practical side of life, to the definite exposition of the contents of all the philosophical systems of the Greeks. Hence we could ill spare the ponderous banter of Cicero when he mocks at the weak points of the Atomic Philosophy,‡ and still less could we spare that reasoned elaboration of its strong points which has made the *De Rerum Natura* of Lucretius the most systematic, the most complete, the most earnest, and the most realistic of all the reductions which the Atomic Philosophy has ever received. But after allowing for all his skill in the episodical handling of the rival systems of Heraclitus, Empedocles, and Anaxagoras, for his power of description, for the vivacity of his narrative, for the force and often the beauty of his illustrations and analogies, it must still be conceded that there is much more of original poetry than of original philosophy in these glowing hexameters of the Epicurean philosopher-poet.

In a history of the Atomic Philosophy we can leap the chasm of the Middle Ages at a single bound. The physical philosophers of

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* *Phædo*, § 47; Jowett's Plato, vol. I, p. 427.

† For evidence as to the imbecility of the Roman mind in physical philosophy, see the 2nd Book of Cicero's "*Prior Academics*," which is a long wail over the want of truth, or of tests of truth, in physical speculation.

‡ *De Natura Deorum*, I, 18, 54, 66, 69, 73, 120; *cf. De Fato*, I, x, xi, xx; *De Finibus*, I, vi–vii; *Tusc. Disput.* I, xi, 22; xviii, 42.

that time were not discussing the concourse of atoms, fortuitous or otherwise, but were carefully pondering, with Doctors Divine and Angelical, Subtile and Irrefragable, the difference between *Ens* and *Essentia*, between *materia quomodolibet accepta* and *materia signata*, between *quidditas per se* and *hæcceitas per se*, between *ultima entitas entis* and *ultima actualitas formæ.* As we plod our weary way through the *Quodlibeta* of these venerable doctors, we can but envy the angels one of the faculties ascribed to them by St. Thomas Aquinas—that of being able to pass from point to point without passing through intermediate spaces.

Bacon,* as he stood at the threshold of the new dispensation of physical science, had made a plea for the forgotten philosophy of Democritus, but when the metaphysical philosophy of Europe came to a new Avatar in the brain of Descartes, we find that thinker denying a discrete conception of matter, and arguing for the contrary conception of continuous extension, of the identification of extension with substance, and, hence, of the infinite divisibility of matter. He says: "It is easy to demonstrate that there cannot be atoms; that is, parts of bodies or of matter which are of an indivisible nature, as some philosophers have imagined, since, however small we may suppose these parts, inasmuch as they must needs have extension, we conceive that there is not one of them which cannot still be divided into two or more still smaller parts; whence it follows that it is divisible." † It will here be seen that Descartes falls into a confusion of ideas with regard to the atoms of the ancient philosophers. They did not conceive that the atom was indivisible because of its smallness, but because of the indestructible solidity which made it incapable of being cut, or broken, or bent, and which also made it impervious to heat or humidity. ‡

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* See, especially, *Cogitationes de Natura Rerum*, and *De Principiis atque Originibus*, &c. Works, (Ellis & Spedding's ed., London,) vol. III, pp. 15, 82, *et seq.; cf.* Advancement of Learning, Book II, vii, 7, (Ellis & Spedding's ed.,) vol. III, p. 358.

† For a formal criticism on Democritus' theory of atoms see *Principes de la Philosophie*, *Œuvres de Descartes*, (Cousin,) tome III, p. 516, and *cf.* Aristotle: *De Generatione et Corruptione*, I, ii, 11–21, where this criticism is anticipated and surpassed.

‡ "*Corpora individua propter soliditatem*," Cic., De Fin., I, vi, 17; *cf.* Lucret., I, lines 532–5.

And this supposed conflict between the infinite divisibility of matter, mathematically considered, and the actual indivisibility of atoms, physically considered, is a pure logomachy resulting from what the lawyers would call a misjoinder of parties and a misjoinder of issues. The mathematician, contending for the infinite divisibility of matter, proceeds from the idea of space to a fact in nature, while the atomist, contending for the actual indivisibility of the atom, proceeds from an assumed fact in nature to the idea of space, and so, as has been said, the duellists cross swords in the air over the head of a phantom standing between them, and never succeed in touching each other.*

From this time onward, for many years, the opinions of philosophers concerning the nature or reality of atoms seem to have floated in a state of uncertainty between the views of the ancients and the views of Descartes. For instance, we find Henry More, the platonizing metaphysician of England, in the 17th century, adventuring the following dogmatic definition of matter: "I have taken the boldness to assert that matter consists of indiscerptible parts, understanding by indiscerptible parts particles that have, indeed, real extension, but so little that they cannot have less and be anything at all, and, therefore, cannot be actually divided. The parts that constitute an indiscerptible part are real, but divisible only intellectually, it being of the very essence of whatever is to have parts or extension in some measure or other, for, to take away all extension is to reduce a thing only to a mathematical point."†

For the physical atom of Greek metaphysics, Leibnitz, it is known, substituted the monad or formal atom, considered as the continent and complex of an infinite number of essences. Leibnitz tells us that so soon as he had thrown off the yoke of Aristotle he plunged into the vacuum and atomic hurly-burly of Democritus, but that he could find no rest there, because he could not account for the genesis of mind in man on any mechanical theory of purely physical atoms. Hence the invention of the Leibnitzian Monadology and Pre-established Harmony—a form of metaphysical philosophizing which reflects the mental evolution and intellectual environment

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*See Westminster Review, vol. 59, p. 178, *cf.* Samuel Brown: Lectures on the Atomic Theory, Edinburgh, 1858.

† Quoted in Munro's *Lucretius*, vol. 2, p. 158.

of the 17th century, as exactly as the metaphysical speculations of Anaxagoras and Democritus and Epicurus represent the mental evolution and intellectual environment of the Greeks two thousand years before. The atom of Leibnitz instead of being an indivisible and sempiternal "solid singleness" is a created monad, a "manufactured article," deriving its perpetuity and power from the immanent and perpetual "fulgurations of Divinity."* It is the curious destiny of the atomic philosophy, let me here say, in parenthesis, that it has subtended three very distinct orders of metaphysic—the polytheistic metaphysic of Greece and Rome, the theistic metaphysic of the Renaissance period, and the scientific metaphysic of our own day.

According to Leibnitz separate classes of monads have separate qualities. Different degrees of aggregation in monads, differing in kind, make the varieties of matter. The natural mutations of monads proceed from an intestine force which is the principle of change in matter. In all simple substance there is a plurality of relations and affections, so that a residuum of relations and affections remains after every transfer of affections and relations in the production of material changes. These material changes proceed according to the law of continuity, for all change of the created monad is only the modulus of its perdurancy.† The monad is inconceivable, except as a creation of Divinity. When created, it is destructible only by the decree of Omnipotence.‡ Monads work no changes in each other's inner constitution, and therefore act on each other according to a Divine Pre-established Harmony, which makes each monad the mirror of the Universe, and the continuous register of all physical changes, past, present, and future. Hence the order of the movement and the continuity of the process which have resulted in the formation of the only world possible and the best world possible. It will be seen that we are here

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* "*Dieu seul est l'unité primitive ou la substance simple originaire, dont toutes les monades créées ou dérivatives sont des productions, et naissent, pour ainsi dire, par des fulgurations continuelles de la Divinité, de moment à moment.*" (*Monadologie, prin.* 47). *Leib.: Opera Phil., Lat. Gal. Germ. omnia.* Berlin, 1840, p. 708.

† *Leibnitii Principia Philosophiæ, More Geometrico demonstrata*, p. 33.

‡ *Ibid.*, pp. 71, 74.

far enough away from the Epicurean atoms, but we are still working with the atoms of pure metaphysics.

It is equally in accordance with the chronological order of time, and the logical order of scientific ideas, that we should next turn to Newton. And of Newton, the greatest name in all physical philosophy, it need only be said that in his work on Optics he returned to a conception of atoms, which, except that it proceeds on the assumption of a Deity and of final cause, is substantially identical with that of Leucippus, Democritus, and Epicurus. He says: "All *these* things considered [that is, the chemical facts he had just recited], it seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, movable particles, of such sizes and figures, and with such other properties and in such proportion to space as most conduced to the end for which He formed them; and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break in pieces—no ordinary power being able to divide what God himself made one in the first creation." This definition reminds us of Lucretius.

In continuation Newton adds: "While the particles continue entire they may compose bodies of one and the same nature and texture in all ages; but should they wear away or break in pieces, the nature of things depending on them would be changed. Water and earth composed of old worn particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And, therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations, and motions of these permanent particles."

The very form of this last-cited statement carries us back to the cradle of the Atomic Philosophy.* But it is not so much the form of Newton's statement which excites our admiration as the connection of thought in which it stands. The whole of

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* *Δημόκριτος δὲ καὶ Λεύκιππος ποιήσαντες τὰ σχήματα, τὴν ἀλλοίωσιν καὶ τὴν γένεσιν ἐκ τούτων ποιοῦσι, διακρίσει μὲν καὶ συγκρίσει γένεσιν καὶ φθοράν, τάξει δὲ καὶ θέσει ἀλλοίωσιν.* Aristotle: *Περι Γενεσεως καὶ Φθορας*, I, 2, 4. (Didot's ed., vol. 2., p. 434.)

the "31st Query," under which this passage occurs in the book of "Opticks," is occupied with certain chemical analyses which Newton had made in his laboratory. Newton, we know, was an alchemist, and spent laborious days and nights in trying to discover the secret by which base metals might be rendered noble; but I can hardly concur with Prof. Jevons when he says that Newton's "lofty powers of deductive investigation were wholly useless" in the conduct of these experiments.* There is some gold at the bottom of even his alchemical crucible. He was the first to put the conception of atoms in their rightful logical connection with the phenomena of practical chemistry.†

It would here be in order to follow Joseph Boscovich in his profound theory of the constitution of matter, if in doing so we might not fall into the danger of drifting too far from the atom considered as a minim of corporeal singleness. With him the atom is a point of attractive and repulsive forces rather than an ultimate physical element; and as it was really the atom of chemical physics which Democritus posited in his mind without knowing it, thus setting up the altar of science to an "unknown god," it is time that we should hasten towards the epoch when Chemistry came to rend the vail from the face of this Isis whom the Greek atomists had so long and so ignorantly worshipped.

It is in the writings of the Hon. Robert Boyle, pleasantly described by his Irish biographer, with a somewhat Irish collocation of ideas, as "Father of Chemistry and brother of the Earl of Cork," that we find the period of transition, when the old order of metaphysical atoms is changing to give place to the new order of physical atoms as weighed and measured by modern chemistry. In his essay on "The Intestine Motions of the Particles of Quiescent Bodies," ‡ as also in his essays on Fluidity and Firmness, he threw out some positive ideas on the old atomic philosophy. He supposes it to be of Phœnician derivation, and even tries to effect a reconciliation between that philosophy and the Cartesian notion of continuous substance by drawing on the *materia subtilis* of the French philosopher (which was conceived to pass constantly, like a

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* Jevons: Principles of Science, vol. II, p. 133.

† Opticks, Book III, Query 31.

‡ Robert Boyle's Works, vol. I, p. 444.

stream, through the pores of the solidest matter) as a very good analagon for the racing atoms of Epicurus.

It is, however, in his essay entitled an "Attempt to make chemical experiments useful to illustrate the notions of the corpuscular philosophy,"* that he approaches this discussion with a bold front. He there says: "The corpuscular doctrine, rejecting the substantial forms of the schools, and making bodies to differ but in magnitude, figure, motion, or rest, and situation of their component particles, which may be always infinitely varied, seems much more favorable to the chemical doctrine of the possibility of working wonderful changes and even transformations in mixed bodies. . . . As many chemical experiments may be happily explicated by the corpuscularian notions, so many of the corpuscularian notions may be commodiously either illustrated or confirmed by chemical experiments."†

It will be seen at once, in the very dialect and purport of such language, that we have reached, even in Boyle, a turning point of the whole Atomic Philosophy. His words import that we are to use "the corpuscularian notions" to explicate *chemical* experiments, and that, in turn, the corpuscularian notions may find a new and solid basis in chemical *experimentation*. Men have changed their whole *Welt-Anschauung*, as compared with that of the Greeks in the days of Epicurus, before such processes of thought and such instruments or methods of enquiry become possible. It is only as the thoughts of men are widened with the process of the suns that they take in, or can take in, those wider horizons and deeper vistas of truth which are opened to the human mind by the ascending hierarchies of the physical sciences. We have now passed the border-line which separates the metaphysico-physical atoms of Epicurus from the physico-metaphysical atoms of modern chemical science.

I can afford to pass over this part of my story *sicco pede*, for we shall henceforth have to deal only with the atoms required by the hypotheses of positive and experimental science to explain the actual facts and processes of nature, not as those facts and processes lie on the surface of things, but as extorted from the very bosom

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* Robert Boyle's Works, vol. I, p. 354.

† *Ibid.*, pp. 358, 359.

of nature by the racks and thumbscrews of physical enquiry. Instead of taking our atoms as they were distilled and attenuated by the refining brains of an Anaxagoras, a Democritus, an Epicurus, or a Leibnitz, we can now take them as weighed and measured by the quantitative, qualitative, or volumetric analysis of modern chemistry, in ways that Anaxagoras or Democritus or Epicurus or Leibnitz never dreamed of in their philosophy. The distance between the 5th century before Christ and the year 1800 is measured as well by John Dalton as by John Howard. John Dalton and John Howard would have each been impossible in the days of Democritus—the one as much so as the other. John Howard plunged into the reek of European prisons at the impulse of a Christian philanthropy unknown to the Greek, with all his love of the Good, the Beautiful, and the True. John Dalton plunged into the reek of the Lancashire marshes,* at the impulse of an abstract science unknown to the Greek with all his love of dialectics, of art, and of æsthetic culture. John Howard, to use the fine phrase of Burke, taught men who love mercy to "take the gauge and dimension" of human misery. John Dalton taught men who love truth in disinterested studies to take the gauge and dimension of the elements which compose the physical Universe. Who was this John Dalton that stands in such typical relation with the scientific thought of our century?

Given, a man "meditative and ratiocinative;" a meteorologist, curious in all eudiometrical research, and, therefore, perpetually experimenting on the constitution of mixed gases; a teacher of arithmetic, so given to mental numeration that on his first visit to London he counts all the carriages he sees while wending his way to the Friends' Meeting House on a Sunday; a chemist, who took the diffusion and absorption of elastic fluids as his special province of investigation; a theorist, who never theorized without an experiment, and an experimenter who never experimented without a theory, and you have John Dalton, the father and founder of the Modern Atomic Philosophy.

As early as the year 1802, in some experimental combinations of oxygen with nitrous gas, Dalton discovered that "the elements of oxygen combined with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity." Though he

* He obtained his inflammable gas from these marshes.

called attention at the time to "the theory of the process," he does not seem to have apprehended the generality of the principle of definite and multiple proportions till a few years later, when the doctrine dawned on him in the course of some investigations into the constitution of olefiant gas and carburetted hydrogen gas.*

Richter, before him, had ascertained the quantity of any base required to saturate one hundred measures of sulphuric acid, and had formed a table exhibiting the proportions of the acids and alkaline bases constituting neutral salts, but Dalton took this table and translated it into the relative weights of the ultimate atoms composing these saline compounds.†

The doctrine of atomic weights had thus already become a working hypothesis in chemistry, no longer an idle speculation, and we soon find Berzelius writing to Dalton that "multiple proportions are a mystery without it." ‡

From this time onward the history of chemistry has been studded with fresh confirmations of the new atomic logic, while ever and anon prophetic glints of truth, implicit in every true physical hypothesis, have leaped into the light of ocular demonstration with each advancing stage in chemical science. Time would fail to tell the beads of the atomic rosary. The doctrine of fixed, multiple, and volumetric combinations, as formulated by Avogadro in 1813; § the determination of the proportions in which bodies combine according to the number and disposition respectively of their molecules, as announced by Ampère in 1814, with special reference to the clear-cut distinction between molecules and their integrant atoms, (already presaged before Ampère by Laurent and Gerhardt;) || the relation between the atomic weights of bodies and their specific heats, conjectured by Dalton and established by Dulong and Petit in 1819;¶ the law of isomorphism, announced by Mitscherlich at the close of the same year, from which it appeared that "a similar atomic constitution determines not only

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*Henry: Memoirs of the Life and Scientific Researches of John Dalton, p. 80.

† *Ibid.*, p. 85.

‡ *Ibid.*, p. 100.

§ Wurtz: The Atomic Theory, p. 36.

|| Annales de Chimie, vol. 90, p. 43.

¶ Wurtz: The Atomic Theory, p. 52.

the analogy of chemical properties, but also the similarity of physical forms;" * the discoveries in electrolysis, with their bearing on atomicity, as published by Faraday in 1834, in the Seventh Series of his Experimental Researches; † the labors of Berzelius in clarifying the atomic weights of the elements; the "law of Octaves," announced by Newlands in 1865, according to which the elements were divided into groups, having numbers differing by seven, or some multiple of seven; ‡ the enlarged Periodic System of the elements, as published by Mendelejeff in 1869, with the prognostication of undiscovered metals required to make the system complete—among them a metal which the Russian chemist proceeded to name "ekaaluminium" in advance of its discovery;§ the discovery of the missing metal in 1875, by Lecoq de Boisbaudran, who found it in a blende from the mines of Pierrefitte, in the Pyrenees, and gave to it the name of "Gallium," without knowing that he had lighted on the "missing link" of Mendelejeff; || the extension of this periodic system by Lothar Meyer, with his Curve of the Elements, showing that the ductility, fusibility, and volatility of bodies are functions of their comparative atomic weights; the periodic system, as revised and extended during this very year, by Prof. Carnelley, in the light of the experimental boiling and melting points and heats of formation of the halogen compounds of the elements,¶ (chlorides, bromides, and iodides;) Carnelley's tables of color relations in chemical compounds as indicating the influence of atomic weights;** and, lastly, Carnelley's new reduction of the periodic system of the elements considered in the light of their occurrence in nature, with the helpful inferences to be drawn from it ***—these, and such like discoveries as these, following in the wake of the modern atomic

* Experimental Researches in Electricity, vol. I, pp. 230–258.

† Wurtz: The Atomic Theory, p. 58.

‡ Newlands: The Discovery of the Periodic Law, &c., p. 14.

§ *Annalen der Chemie und Pharmacie, Supplement Band* 8, p. 133 *et seq.*

|| *Comptes Rendus*, t. LXXXI, p. 493. How fully Mendelejeff recognized in gallium the characters wanted to fill the gap in his periodic system, see *Comptes Rendus*, same volume, p. 969.

¶ Philosophical Magazine for July, 1884.

** Phil. Mag. for August, 1884.

*** Phil. Mag. for September, 1884

theory, have abundantly vindicated its value as an instrument of chemical research, while conspiring to vindicate its truth by giving to its votaries that ability of prediction which is the crucial test of science. The theory, besides, has sometimes "snatched a grace beyond the reach of art" by working retroactively to the purification of chemical method from errors and defects incident to the most careful manipulations of the practical chemist.

Standing in the presence of chemical science, as now constituted, Baron Liebig has expressed the opinion that we can scarcely conceive how it could have been developed without the Daltonian hypothesis. And yet the atom of Dalton, considered in its relation to our natural senses, is just as incapable of visible and tangible demonstration as the atom of Democritus. For this reason it is known that Faraday could never fully reconcile himself to the modern doctrine of atoms.* But, in fact, there is a genetic and a generic difference between the ancient and the modern conception. The former is the offspring of the philosophical imagination toying with analogy. The latter is the offspring of the philosophical imagination gendering with the homologies of reason. The atom of Democritus sprang into thought under the plastic forms by which he figured to himself at will the invisible relations and constitution of matter. The atom of Dalton sprang into thought from a rigid mathematical mind figuring to itself certain determinate relations which had become visible in elastic fluids. The atom of Democritus was, by the terms of its genesis, incapable of verification. The atom of Dalton was, by the terms of its genesis, capable of verification, if true, in all the gases of nature. Metaphysic thought born of the analogical reason can never conclusively prove its legitimacy. Metaphysic thought born of the homological reason can always prove its legitimacy, and, until it does, has no rights of heirship in the kingdom of science. The essential quality of a metaphysico-physical hypothesis is that it should be plausible; the essential quality of a physico-metaphysical hypothesis is that it should be apodictic. The former is "magistral and peremptory;" the latter is "ingenuous and faithful." The former is contrived in such sort as to be "soonest believed," the

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* Faraday: Experimental Researches in Electricity, vol. 2, p. 284. But *cf.* vol. I, p. 249.

latter is contrived in such sort as to be "easiliest examined," to cite the words of Bacon.*

The Atomic Philosophy may, therefore, be said to offer a good type of all that is valid in physical metaphysics, and of all that is invalid in metaphysical physics. As the child in the infantile stage of his development dwells delightedly amid fays and talismans, because his metaphysic is stronger than his physics, so the savage man, artless child of nature, is easily pleased with the rattle of some lying legend, or tickled with the straw of some preposterous myth—the more preposterous the better. A cultivated race whose imagination is creative and artistic, but whose reason has not yet been developed by the processes of a rigorous logic, will demand, as has been already said, an artful and curious felicity in their physical theories—but they will demand nothing more, because when this demand is met, their highest intellectual demand has been met. It is not until "the heir of all the ages" has learned to change the organon and method of his physical enquiries, and to put his reason over his imagination, by making imagination the hand-maid of reason, that Science is born. Long before this stage has been reached the children of Science may come to the birth, but there is not strength to deliver, because the true *maieutic* of science—experimentation with rational hypothesis, and rational hypothesis with experimentation—has not yet come to the teeming mind of philosophy. The goddess Experimentation is the Lucina of Science. The free surrender of all metaphysical conceptions to the hands of this Lucina, with the distinct knowledge that she will strangle them if they are not well formed, is the birth-pang of the scientific spirit. Until this stage of mental evolution is reached we shall have as many theories of the Universe as we have stages of culture, for every stage of culture will have a physics of its own, because it has a metaphysic of its own. Hence, the endless varieties of cosmology—the Hottentot physics, the Indian physics, the Stoical physics, the Epicurean physics, the Leibnitzian physics, the Cartesian physics, and such like—all the coinage of the metaphysical imagination. Grote enumerates as many as twelve distinct physical philosophies which divided speculative opinion in Greece during the century and a half between Thales and the Peloponnesian war.

* The Advancement of Learning, Book I, v, 9.

It is the mission of science to bring the physics of the world into unity by reading the phenomena of the world in the dry light of reason, and by continuing to spell and parse the hieroglyphs of Nature until the rational processes of our logic are brought into demonstrated correspondence with the actual processes of Nature. Science still keeps metaphysic in her service. But instead of weaving whole fabrics from the metaphysical loom and devising ingenious tissues which only reveal the nakedness of reason, Science in passing from the known to the unknown employs metaphysic as the gossamer spider employs the single thread on which she sways and balances her movements between two solid points. The thread is tied to something solid as the condition of reaching something solid after her aerial flight. So the man of science, working in and under the limitations of physics, works on the lines of metaphysic thought when he frames the tentative hypotheses with which he returns again to the patient, practical study of nature.*

The scientific man reads the Universe backward by the inductive syllogism, because Nature has proceeded forward in her evolutions, according to an unbroken chain of antecedent causes. The physical Universe is indeed a fasciculus of natural syllogisms colligated into the compactest unity, and so holding all things, forces, and functions under the bonds of logic. The scientific man, at any given stage of his enquiry, has before him only the conclusions or at best only the minor premises and the conclusions of this world-process. And he knows that these conclusions of the natural syllogistic process have been reached through a perpetual flux in the universal complex of things, forces, and functions—a flux which dates from the beginning of star-mist and nebula, or from the beginning of that more elementary fluid out of which star-mist and nebula were generated, according to the scientific metaphysic of the present day. Is it any wonder, then, that many of the major premises of Nature's physical syllogisms should still be wrapt in impenetrable mystery to us, as many of the major premises which

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* Bacon's oft-quoted contrast between metaphysicians, who, he says, spin "laborious cobwebs of learning," like spiders, and physical philosophers, who "work according to the stuff, and are limited thereby," seems hardly fair to the spider. *Advancement of Learning*, Book I, iv, 5.

we have spelled out were wrapt in an impenetrable mystery to the Greeks in the 5th century before Christ?

As there is a needs be that much of metaphysic thought must be blended with the psychological processes which lead to every passage from the known to the unknown, so every great discovery of the physical philosopher tends to widen the metaphysical horizon within which he works. The world was never so full of metaphysic as it is to-day, when physical science is transforming the minds of men not so much by the secular boons it is dropping in the lap of modern civilization as by its underlying doctrines; and these doctrines are often the mere metaphysical reflex or obverse of the physical truths they subtend. The psychological processes of every age are conditioned by its logical method, and its logical method is justified to itself by its metaphysic—by those necessary conceptions and fundamental relations which it takes to be architectonic of the Universe. What, for instance, can be more metaphysical than the latest conception of our highest physical science—the conception of vortex atoms moving in an imaginary frictionless fluid where the origin and the end of the motion are equally inconceivable? Or, take Mr. Darwin's doctrine of hypothetical gemmules "inheriting innumerable qualities from ancestral sources, circulating in the blood and propagating themselves, generation after generation, still in the state of gemmules, but failing to develop themselves into cells because other antagonistic gemmules are prepotent and overmaster them in the struggle for points of attachment"*—in what respect is this doctrine one whit less metaphysical than St. Augustine's doctrine of original and hereditary sin? Or, when the late Prof. Clifford tells us that "the Universe consists entirely of mind-stuff;" that "matter is a mental picture, in which mind-stuff is the thing represented," and that "reason, intelligence, and volition are properties of a complex which is made up of elements themselves not rational, not intelligent, not conscious"—how does his "mind-stuff" differ from the "mind-stuff" of Pythagoras,† except in the

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* Galton: Hereditary Genius, p. 367; *cf.* Darwin: Animals and Plants under Domestication, (London,) vol. 2, p. 402. For a criticism on this physiological doctrine, see Encyclopædia Britannica, ("Atoms,") vol. 3, p. 42.

† For the "mind-stuff" of Pythagoras, see Cicero, *De Nat. Deorum*, I, xi, 27. For the "mind-stuff" of Clifford, see "Mind," January, 1878, p. 66.

greater ingenuity and method of the metaphysic art with which it is conceived?

If within the limits of this discussion I had the time, and if, under the limitations of my knowledge, I had the ability, to carry this enquiry into the realm of molecular physics and dynamics, where such star-eyed mystagogues as a Clausius or a Rankine, a Clerk-Maxwell or a Sir William Thompson have borne the thyrsus of science before us, it would be easy to show that, under their guidance, we have escaped the pitiless parallel lines of the Epicurean atoms only to find ourselves inextricably implicated in the knottedness and linkedness of the vortex rings of atoms as they execute their infinite evolutions and involutions, vibrating now in one period and now in another behind that vail of matter where they can be descried only by the shadowy lines they reveal to the spectroscopic imagination. "It is the mode of motion," says Clerk-Maxwell, "which constitutes the vortex rings, and which furnishes us with examples of that permanence and continuity of existence which we are accustomed to attribute to matter itself. The primitive fluid, the only true matter, entirely eludes our perceptions when it is not endued with the mode of motion which converts certain portions of it into vortex rings, and thus renders it molecular."*

Of these vortex rings we must say, in the dialect of the schools, *cognoscendo ignorantur, sed ignorando cognoscuntur.* Withheld from positive conception, yet necessitated to scientific thought and speculation by the exigencies of the knowledge we *can* conceive positively, they afford a good illustration of the physical metaphysic which has wafted the scientific mind of the present generation into an empyrean as much higher than the empyrean of Plato as the spectroscopic vision of modern science is more far-reaching than the highest flight of metaphysic wit among all the physical atomizers who ever lived or dreamed in Greece. Every chemical atom, says Sir John Herschel, is forever solving differential equations, which, if written out in full, might belt the earth. "An atom of pure iron," says Jevons, "is probably a vastly more complicated system than that of the planets and their satellites."

Between metaphysical physics and physical metaphysics there is a world-wide difference. The invisible ether posited behind the

* Encyclopædia Britannica, *sub voce* "Atom."

vail of matter by the East Indian philosophy of the Upanishads, or by the visionary dialectic of Cleanthes, was posited there by metaphysical physics. The invisible fluid posited by modern science behind the vail of matter is posited there by physical metaphysics. The vortices of Democritus as well as the vortices of Descartes are the creations of metaphysical physics. The vortices of Helmholtz and of Sir William Thompson are the creations of physical metaphysics. The fixed and crystalline sphere of the old Ptolemaic astronomers was an invention of metaphysical physics. The solid ether which transmits to us the light of the stellar Universe, and which, as Sir John Herschell remarks, is the modern "realization of the ancient idea of the crystalline orb," is the invention of physical metaphysics. When Lucretius finds in the iridescent hues of the peacock's tail, as it shimmers in the sun, a fresh type and instance of Nature's prodigality in the display of atoms, he does but yield another contingent to the barren store of his metaphysical physics. When Dr. John Tyndall finds in the iridescences of the common soap bubble a proof that stellar space is a plenum filled with a material substance that is capable of transmitting motion with a rapidity that would girdle the equatorial earth eight times in a second, he does but yield another contingent to the fertile store of his physical metaphysics. When Dr. George Cheyne, of Scotland, expressed the opinion in the last century, that "all animals, of what kind soever, were originally and actually created at once by the hand of Almighty God, it being impossible (he said) to account for their production by any laws of mechanism;" and when he further held that "every individual animal has, *in minimis*, actually included in its loins all those who shall descend from it, and every one of these again has all its offspring lodged in its loins, and so on *ad infinitum*," and that "all this infinite number of animalcules may be lodged in the bigness of a pin's head,"* he preached a biological doctrine which sounds in the terms of metaphysical physics. When Mr. Darwin in his provisional theory of Pangenesis assumes the existence of the gemmules which inherit innumerable qualities from ancestral sources, and which prelude as gemmules that struggle for existence which antedates and therefore conditionates the terms of the human struggle witnessed in society, commerce, and national life, he expounds a biological doctrine which sounds just as clearly in the terms of physical metaphysics. When old

* J. Brown: Locke and Sydenham, p. 270.

Heraclitus proclaimed that the Universe with all it contains sprang into being from elemental heat, and was destined to be resolved again into the elemental heat from which it sprang, and thus in a ceaseless round to continue the cycle of being, he taught a doctrine of conservation and correlation of energy which had its root in metaphysical physics. When Dr. John Tyndall declares that "all our philosophy, all our poetry, all our science, all our art—Plato, Shakespeare, Newton, and Raphael—are potentially in the fires of the sun," and so tucks away the genius of a Darwin in the folds of a nebular blastema, he teaches a doctrine of equivalence which has its root in physical metaphysics.

It will thus be seen that under the dominion of Science the world has use for as much metaphysic as ever before, but only for a metaphysic radically different from the old metaphysic in its point of departure as also in the tests of its validity, and, therefore, radically different in the tenure by which it is held. The votaries of the old metaphysical physics proceeded from what was unknown to explicate and explain the known appearances of things, and rested content in explanations which seemed to consist with those appearances. The votaries of the modern physical metaphysics proceed from what is known to explicate and explain what is unknown in the deeper relations of things, and rest content in explanations only so long as, and so far as, they seem consistent with experimental proofs or with the broadest homologies of the deductive reason.

When the law of simple multiples in chemical combinations was given to the world by Dalton, and was expressed by him in atomic language, he had really made a great departure from the physical methods of Democritus, though it is curious to observe that there is a perfect identity between the metaphysical ideas underlying his logic and the metaphysical ideas of his Greek predecessor. The method of each proceeds on the assumption of the indestructibility of matter, and it is from this platform that the English chemist reaches out his hand to the Greek philosopher in token of a common metaphysic. "No new creation or destruction of matter," wrote Dalton, in his celebrated paper on "Chemical Synthesis," "is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydro-

gen." * Democritus knew nothing of hydrogen, but he saw as clearly and said as plainly as Dalton that the antecedent premise of all physical philosophy must be found in the metaphysical maxim that "out of nothing nothing comes, and that nothing which is can ever be annihilated." †

And this maxim, with which the old Greek philosophy began, is about all of solid and sound that remains to us from the physical philosophizing of the ancients. It is true, as Mr. Balfour Stewart remarks, that the ancients had in some way grasped the idea of the essential unrest and energy of things; that they had the idea ot small particles or atoms as the constituent elements of matter, and divined the existence of an ethereal medium extending through all space; but there is no evidence at all to support the statement that any one or all of these doctrines proceeded from even a rudimental conception of "the most profound and deeply seated ot the principles of the material universe."

There is, however, one respect in which it may be justly said that Democritus stands at the head of the long line of natural philosophers who since his day have been explicating for us the structure of the physical universe. He was the first who ever attempted a purely mechanical solution of the problem of physical being. It is the singular glory of the atomic philosophers that alone, among the jarring schools of Greece, they saw that a science of the Universe was possible only on the assumption that the phenomena of the physical universe are bound together by necessary law, and this law mechanical in the modes of its operation. They had no *science*, it is true, in the modern sense of the word, but it is no small distinction which they have won in standing at the head of an intellectual succession which embraces in its ranks a Copernicus and a Galileo, a Newton and a Laplace, a Dalton and a Faraday. ‡

* Henry: Memoirs, &c., of Dalton, p. 88.

† Diog. Laert., *sub voce* "Democritus," where it is particularly recorded that he assumed as his point of departure the maxim "Out of nothing nothing comes," &c.

‡ "Was die Atomiker von ihren Vorgängern unterscheidet, ist nur die Strenge und Folgerichtigkeit mit der sie den Gedanken einer rein materialistischen und mechanischen Naturerklärung durchgeführt haben; diese kann ihnen aber um so weniger zum Nachtheil gedeutet werden, da sie damit nur die Schlüsse gezogen haben welche durch die ganze bisherige Entwicklung gefordert, und wozu in den Annahmen ihrer Vorgänger die Vordersätze gegeben waren." Zeller: *Philos. d. Griechen*, Erster Theil, 765.

With two short lessons cited to point the moral of this long story, and I have done. The first of these moralities shall be a warning against the folly of the old atomists in attempting to philosophize beyond the conditions of their knowledge. They reared imposing fabrics in astronomy, in physics, in psychology, and in anthropology, but they built without laying their foundation in any deep knowledge of nature, and laid the successive courses of their system-building in the untempered mortar of an incoherent logic. And the moral needs to be pointed as much for the admonition of modern scientific workers, with their cheap and easy cosmologies, as for the reproach of the old physiologers of Greece. One of our poets has sung:

From an old English parsonage
  Down by the sea,
There came in the twilight
  A message to me.
Its quaint Saxon legend,
  Deeply engraven,
Hath, as it seems to me,
  Teaching from heaven;
And all through the hours
  The quiet words ring,
Like a low inspiration,
  "Doe the nexte thynge."

The message is as full of inspiration for guidance in physical philosophizing as for guidance in moral conduct. *Tantum series juncturaque pollet.*

The only other morality which time permits to be pointed at the end of this review is a warning against intellectual impatience—not that intellectual impatience rebuked by the maxim just cited, and which seeks to leap at a single bound the limitations of knowledge in any given age—but the intellectual impatience which cavils at the short-comings of the men who dug the first ditches and planted the first hedges around the vineyards of science. They were humble pioneers, but they opened the way into that land of Beulah where the men of science sit to-day beneath their own vines and fig-trees, with none to make them afraid. Even after John Dalton had come to place the key of the new Atomic Philosophy in the hands of men, it was a saying of Mitscherlich that it took fourteen years to discover and establish a single fact in

chemistry. Let us not wonder, then, that it took more than two thousand years to perfect the doctrine of atoms as a clew to the "mystery of matter." Democritus invented a mechanical key of wonderful ingenuity, but it would not unlock anything that could not be unlocked without it. Newton divined that the key must be fitted to the two great wards of chemical attraction and chemical repulsion, but still the key would not turn in the adamantine lock of Nature. Dalton found that the secret of the combination must be sought in wards nicely graduated according to certain fixed, definite, and multiple numbers, and, since his day, door after door in the chemist's "chamber of imagery" has seemed to swing open at the touch of this talisman. And even, if in the next two thousand years, or in the next twenty years, the theory of John Dalton should be absorbed in some deeper truth, there will still be room in the pantheon of science for the memorial bust of the plain Manchester arithmetician, so long as men recall how far that little candle, which he lighted with inflammable gas obtained in the rudest way from the ponds of Lancashire, has thrown its quickening beams across the whole tract of modern chemistry.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

GENERAL MEETING.

BULLETIN

OF THE

GENERAL MEETING.

244TH MEETING. JANUARY 5, 1884.

The President in the Chair.

Twenty-eight members and guests present.

The Chair announced the death, since the last meeting, of General A. A. HUMPHREYS, one of the founders of the Society.

Mr. J. R. EASTMAN made a communication on

THE ROCHESTER (MINNESOTA) TORNADO,

describing the ground as it appeared a few days after the storm, and showing that the phenomena did not indicate cyclonic motion. All disturbed objects were thrown in essentially the same direction, and were pressed down rather than lifted.

Mr. ELLIOTT related that twenty-five years previous he had crossed a storm-track consisting of a double line of fallen timber, with an interval in which the timber was standing. Mr. EASTMAN thought this phenomenon should be referred to two separate cyclones, possibly moving as companions.

Mr. DALL described storm tracks in the Escanaba region in which the trunks of prostrate trees pointed uniformly in one direction, the path of destruction being definitely limited at the margins.

Mr. E. FARQUHAR suggested that a highly inclined storm axis might account for the uniformity in the direction of the wind in the zone of destruction.

Mr. W. H. Dall read a paper on

RECENT ADVANCES IN OUR KNOWLEDGE OF THE LIMPETS,

summarizing the researches of Spengel on the sensory organs or osphradia; Cunningham on the renal organ and renopericardial pore in *Patella* and *Patina;* Fraissé on the eye in *Patina*, *Fissurella* and Haliotis, and the speaker on the presence of an intromittent male organ in *Cocculina.* He stated that among the *Acmæidæ* and *Patellidæ* the type of eye differs, and while in *Patina* it is of a very rudimentary character, in other genera it might be well developed, as, for instance, in *Ancistromesus*, which has as well developed eyes as *Fissurella.* He also alluded to the gradual progress in classification afforded by anatomical investigation during the past few years, and observed that nearly all the known forms except *Propilidium* and *Scutellina* were amenable to classification; our ignorance of the branchiæ in the former, and the dentition in the latter, operating to prevent a final classification in these two cases, until more is known. Those authors who study the embryology and histology usually from a single species, generally ignore the wide differences of adult anatomy between the genera of Limpets, and sow their generalizations on a basis of classification which is little in advance of that of Lamarck and his immediate successors.

Professor C. H. Hitchcock being present was invited by the Chair to address the Society, and responded briefly.

The President of the Society then pronounced a brief eulogy on General Humphreys, characterizing him as a man who had left behind him an honorable name as well for his distinguished achievements in science and in war as for the virtues and graces which adorned his private life. Mingling among his fellow-men with the utmost unobtrusiveness, and as gentle in spirit as he was brave in conduct, he brought the highest intelligence as well as the highest conscientiousness to the discharge of all the duties—scientific, military, and administrative—with which he filled his long and useful life: a life fitly closed by the serenity and peace of his beautiful death.

245TH MEETING. JANUARY 19, 1884.

The President in the Chair.

Forty-five members and guests present.

The Chair read a letter from the Biological Society of Washington inviting the members of the Philosophical Society to attend its meeting of January 25th, for the purpose of listening to the annual address of its President, Dr. C. A. White.

Announcement was made of the election to membership of Messrs. GEORGE EDWARD CURTIS and PATRICK HENRY RAY.

Mr. I. C. RUSSELL made a communication on

THE EXISTING GLACIERS OF THE HIGH SIERRA OF CALIFORNIA.

[Abstract.]

During the summer of 1883 I had an opportunity of tracing to their sources some of the ancient glaciers of the High Sierra in the region between Mono Lake and the Yosemite Valley.

From the glacial records seen during a number of excursions into the mountains it was evident that the High Sierra had formerly been so deeply covered with ice that only the culminating peaks and ridges escaped the general glaciation. From the vast névé of the mountain tops flowed long winding rivers of ice, both to the eastward and westward through the cañons and valleys. In nearly all cases the glaciers occupied drainage lines of pre-glacial date, which they modified and enlarged, but, with the exception of the cirques about the higher peaks and crests, they failed to originate any of the more prominent topographical features of the range. The glaciers of the Sierra Nevada were not connected with a northern ice-sheet, but were of local origin and of the same type as the Swiss glaciers of the present day, but of far greater magnitude. If the cañons and valleys of the Sierra are traced upward, it is almost invariably found that they head in cirques or amphitheaters, in some of which small glaciers still linger—perhaps remnants of the mighty ice-rivers that formerly flowed from the same fountains.

The first glacier visited by the writer was on the northern side of Mt. Dana, at an elevation of about 11,500 feet above the sea, and at the head of a deep cañon which drains into Leevining creek,

one of the tributaries of Mono Lake. The Mt. Dana glacier is approximately 2,500 feet long and of somewhat greater breadth. Although small, and in fact but a "pocket edition" of what may be seen on a far grander scale in many mountains, yet it is a veritable glacier, with nearly all the features that characterize such ice-bodies in other countries. The distinction between the snow-ice of the névé and the more solid blue or greenish-blue ice of the glacier proper is clearly marked—as was observed to be the case also in a number of neighboring glaciers. An irregular open fissure crosses the head of the névé, corresponding to the "bergschrund" of the Swiss glaciers, while a number of parallel fractures on the border of the glacier at the foot of the snow-field form marginal crevasses with walls of solid blue ice. Near the terminus of the glacier alternating sheets of porous, white ice, and of more compact bluish ice were observed, which produce a distinct laminated or ribboned structure. Dirt-bands were plainly visible, sweeping in undulating lines across the surface of the glacier; and similar bands are a conspicuous feature in nearly all the ice-bodies seen in the High Sierra. About the foot of the Mt. Dana glacier a true terminal moraine is now in process of formation. The fall of stones and dirt from the ice onto the moraine was noticed many times during our visits. Some of the rounded stones from beneath the ice are battered and scratched and have evidently received these markings within the past few years.

On the northern side of Mt. Lyell another glacier was visited, which is the source of the Tuolumne river. The Mt. Lyell glacier is somewhat larger than the one on Mt. Dana, and, like it, exhibits characteristic glacial phenomena. A protrusion of compact, banded ice from beneath a snow-field at the head of an amphitheatre was here again observed, as well as the presence of moraines, crevasses, dirt-bands, etc. On the lower portion of this glacier were observed "ice-pyramids" of the form represented in the figure on the following page.

At the northern base of a pyramid there invariably occurs a stone or a mass of dirt, that is depressed below the general surface of the glacier, as is indicated in the sketch. The pyramid invariably points toward the noon-day sun. Its mass is composed of porous and banded ice, like that forming the general surface of the glacier, but its northern face is sheeted with compact, bluish ice. The

northern face is also concave, as represented in the sketch, and usually conforms to some extent with the shape of the stone at its base.

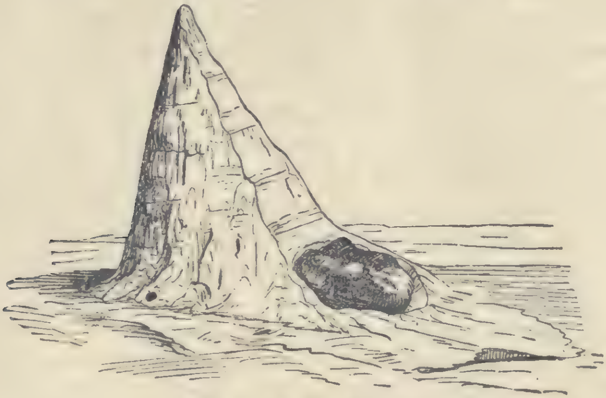

Fig. 1. An Ice-Pyramid.

On another glacier, discovered at the head of Parker creek, one of the tributaries of Mono Lake, all the glacial phenomena mentioned above are well displayed, and, in addition, "glacier-tables" were observed in considerable numbers. The following figure represents several of the glacier-tables of the Parker creek glacier, grouped for convenience of illustration:

Fig. 2. Glacier-Tables.

The largest perched-block now being carried along by this glacier measures 34 by 28 by 10 feet, and is supported on a column of ice five or six feet thick, eight feet high on its northern side, and six feet high on its southern. Many masses of rock larger than the one measured were seen in the terminal moraine that circles about the foot of the glacier.

The motion of these glaciers was not observed, but that it exists is manifest from the nature of the crevasses and the curvature of the dirt-bands. The rate of flow of a glacier on Mt. McClure was measured several years since by Mr. Muir, who found it to be 47 inches in 46 days (from August 21st to October 6th, 1872).*

Six glaciers are known to the writer within the southern rim of the hydrographic basin of Mono Lake, and about twice this number were seen about Mt. Conness, Mt. McClure, Mt. Lyell, Mt. Ritter, and the Minarets.

Many of the glaciers mentioned above have been previously reported in popular articles by Mr. John Muir, but the fact that they are true glaciers having been denied by eminent geologists, it is desirable to have a more accurate description of them.

[The communication was illustrated by photographic lantern views. Its subject-matter will be more fully presented in the Fifth Annual Report of the United States Geological Survey.]

Mr. GILBERT THOMPSON described certain glaciers on Mount Shasta believed to be new to science. Their discovery increases the number of known glaciers on the flanks of Shasta to seven.

Mr. HOLMES described modern glaciers of the Rocky Mountains observed by himself. Those of the Wind River Mountains are from one-fourth mile to one mile in length. He illustrated by a sketch the position of three small glaciers in the gorges of Mount Moran, in the Teton Range, at an altitude of 10,000 feet.

Mr. POWELL remarked that the chief interest of these small modern glaciers lies in the fact that they illustrate the process by which the drift has been distributed, and aid in completing the theory of the ancient glaciation of the country.

Mr. MARK B. KERR mentioned the occurrence of a probable glacier in the Salmon Mountains, a division of the Coast Range.

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* American Journal of Science, Vol. V, p. 69; 1873.

Mr. HARKNESS set forth the apparent difficulty of discriminating between a névé and a glacier proper, and requested that some geologist would define the term "glacier."

Mr. EMMONS said that a true glacier is an ice river, conforming in shape to the more or less restricted channel in which it flows, and this characteristic might form a base of distinction between the true glacier and the névé-field, the latter being comparable to the lake which forms the source of a mountain stream. Thus the névé would become a glacier only when from a broad and shallow ice-field it had become compressed into a narrower and deeper mass, between confining walls.

Other remarks were made by Messrs. E. FARQUHAR, GILBERT, DALL, and ELLIOTT.

Professor W. C. KERR made a communication on

THE MICA MINES OF NORTH CAROLINA.

[Abstract.]

The profitable mines are restricted to a plateau limited eastward by the Blue Ridge and westward by the Smoky Range. These were anciently worked on a very extensive scale. Few other modern mining operations have been so profitably conducted as those at the points occupied by the early miners. The ancient work was performed with blunt-pointed tools—doubtless of stone—and was confined to the partially decomposed portions of the granite veins, but large pits were nevertheless excavated. One of these measures 150 by 75 feet, and, despite a partial filling with débris, retains a depth of 35 feet. Facts connected with the arboreal vegetation show that some, and perhaps all of these openings were abandoned as much as five hundred years ago. The modern industry began in 1868, and, although it has assumed considerable importance, is not yet conducted in a systematic way.

The character of the mica and its associated minerals were discussed and illustrated by specimens.

246TH MEETING. FEBRUARY 2, 1884.

The President in the Chair.

Forty-eight members and guests present.

The Chair announced the election to membership of Mr. THOMAS ROBINSON.

Mr. C. V. RILEY made a communication on

RECENT ADVANCES IN ECONOMIC ENTOMOLOGY.

The paper set forth the part which insects play in the economy of nature, and particularly their influence on American agriculture. The earlier writers on applied entomology in the United States, as Peck, Harris, Fitch, Walsh, LeBaron, Glover, did some excellent work in their studies of the habits and life-histories of injurious species, but the most important results followed when such studies were combined with field work and experiment by competent persons and upon scientific principles. A number of the remedies proposed in the agricultural press are foolish and based on misleading empiricism. Economic entomology as a science is of comparatively recent date. It implies full knowledge of the particular injurious species to be dealt with and of its enemies, of its relations to other animals and to wild and cultivated plants. In short, the whole environment of the species must be considered, especially from the standpoint of the farmer's wants. The habits of birds, more particularly, and the bearings of meteorology and of the development of minute parasitic organisms must be considered. Experiments with insecticides and appliances will then be intelligent and successful in proportion as the facts of chemistry, dynamics, and mechanics are utilized.

The complicated nature of the problem was illustrated by the life-history of *Phylloxera vastatrix* Planchon, and the difficulties often encountered in acquiring the facts were illustrated by the late work on *Aletia xylina* (Say).

The chief insecticides considered for general use and applicable above ground were tobacco, white hellebore, soap, arsenical compounds, petroleum, and pyrethrum; those for use under ground, naphthaline, sulpho-carbonate of potassium, and bisulphide of car-

bon. The most advantageous and improved methods of utilizing each were indicated. Recent experiment showed that kerosene emulsions, such as had been recommended lately in the author's official reports, are superior to bisulphide of carbon when used under ground against the Grape *Phylloxera*, and the discovery is deemed of great importance, especially to the French people and those on our Pacific slope. Contrary to general belief, pyrethrum powder was shown to have a peculiar and toxic effect on higher animals as well as on the lower forms of life. Its deadly influence on lower organisms led the author to strongly recommend its use as a disinfectant, and to express the belief that it will yet come to be used in medicine. Dr. H. A. Hagen's recommendation of the use of yeast ferment was touched upon. It has proved of little or no practical avail, and some of the publications on the subject were characterized as unscientific. The use of malodorous substances as repellants, which was much relied on in the early days of economic entomology and strongly recommended by the two Downings, has lately been agitated as a new principle for the prevention of insect attack by Prof. J. A. Lintner. The principle can be applied in exceptional cases to advantage, but experiment gives little hope of its utility against most of our worst field insects. Prof. S. A. Forbes is engaged in interesting researches, having for object the utilization of micro-organisms, but with more promise for pure than applied science.

Of recent progress in mechanical appliances, the paper dealt with those lately perfected under the author's direction by Dr. W. S. Barnard, one of his assistants. This part of the subject was illustrated by models and by plates from the forthcoming fourth report of the United States Entomological Commission.

The paper concluded with the following plea for applied science: "Matters of fact do not tend to provoke thought and discussion; and I must confess to some misgivings in bringing these practical considerations before a body which reflects some of the highest and purest science and philosophy of the nation. From the days of Archimedes down to the present day there has existed a disposition to decry applied science and to sneer at the practical man. Yet I often think that science, no matter in what fine-sounding name we clothe her, or how high above the average understanding we stilt her, is, after all, but common sense employed in discovering the

hidden secrets of the universe and in turning them to man's wants, whether sensual or intellectual. Between the unbalanced vaporings of the pseudo-scientific theorizer and the uninformed empiric who stumbles upon a discovery, there is the firm middle ground of logical induction and deduction, and true science can neither be exalted by its inapplicability, nor degraded by its subserviency to man's material welfare. The best results follow when the pure and the applied go hand-in-hand—when theory and practice are wedded. Erstwhile the naturalist was honored in proportion as he dealt with the dry bones of his science. Pedantry and taxonomy overshadowed biologic research. To-day, largely through Charles Darwin's influence, we recognize the necessity of drawing our inspiration more directly from the vital manifestations of nature in our attempt to solve some of the many far-reaching problems which modern science presents. The fields of biology, morphology, physiology and psychology are more inviting than formerly. Nor is the lustre that glorifies the names of Stevenson, Watts, Faraday, Franklin, Morse, Henry, Siemens, and a host of yet living investigators dimmed because they made science useful. Goethe makes Wagner say:

"'Ach wenn man so in sein Museum gebannt ist
Und sieht die Welt kaum einen Feiertag
Kaum durch ein Fernglas, nur von Weiten
Wie soll man sie durch Ueberredung leiten?'

"If to-day, right here in Washington, there is great activity in the field of original research; if the nation is encouraging it in a manner we may well be proud of, the fact is due in no small degree to the efforts of those, many of them members of this Society, who have made practical ends a means, rather than to those who would make science more exclusive, and who are indifferent to practical ends or popular sympathy. Such, at least, is my apology for the nature of this paper."

In response to an inquiry by Mr. White, Mr. Riley said that the ox-eye daisy had been subjected to a thorough test under his supervision and the result had shown that it has none of the insecticide qualities of pyrethrum.

Mr. S. M. Burnett made a communication entitled

WHY THE EYES OF ANIMALS SHINE IN THE DARK.*

[Abstract.]

Erroneous opinions have been held and expressed, not only by the non-scientific, but also by some persons holding high positions in the scientific world, in regard to the phenomena of luminosity of the eyes of animals, and particularly of cats, when they are in obscurity. It is not due, as has been commonly supposed, to phosphorescence, but to light reflected from the bottom of the eye, which light is diffused on account of the hypermetropic condition that is the rule in the lower animals.

In response to a question by Mr. White, Mr. Burnett said that human eyes affected by hypermetropia do not yield similar results, partly because the human pupil is too small and partly because the bottom of the human eye is not so strongly reflecting a surface as that of most animals.

Mr. Harkness remarked that in determining the degree of divergence of rays emitted by an eye, from an image situated upon its retina, it is necessary to consider the magnitude of that image as well as its distance from the focal plane of the lens. The divergence of the rays coming from any *one* point of the image is determined by the interval which separates the retina from the focal plane of the lens, while the divergence of the rays coming from any *two* points of the image depends principally upon the size of the image itself. The total divergence is the sum of the divergences produced by these two causes, and the neglect of that due to the size of the image will probably account for the discrepancy between the observed angle of divergence and that computed by Dr. Burnett.

It also seems desirable to bear in mind the distinction between fluorescent and phosphoresent light; the former disappears as soon as the incident waves are cut off; the latter does not.

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* This paper is published in full in the Pop. Sci. Monthly for April, 1884; Vol. XXIV, pp. 813–818.

Mr. A. B. JOHNSON made a communication on

SOME ECCENTRICITIES OF OCEAN CURRENTS.

[Abstract.]

The records of the Light House Board show that no less than eleven buoys of various patterns have gone adrift from the waters of the United States and been found at distant points where ocean currents have carried them. Many of these were not so fully identified that their precise original station could be indicated. In the case of a few, it has been determined that they were swept from the harbor and bay of New York by the outgoing ice in the winter of 1880–81 when nineteen buoys were carried to sea.

1. In the spring of 1871, a buoy was picked up on the west coast of Ireland.

2. In March, 1871, the Norwegian vessel Vance picked up a buoy in lat. 42° 22′, long. 26° 38′.

3. In February, 1881, a buoy went ashore on one of the cays near Turk's island. This was recognized as a New York buoy.

4. May 17, 1881, the steamer William Dickinson passed a whistling buoy in lat. 29° 46′, long. 77° 38.

5. In March, 1881, a buoy of the largest size, likewise referred to New York, was found near Bermuda.

6. In February, 1882, a Sandy Hook buoy was found near Bermuda.

7. In February or March, 1882, a buoy was washed ashore at Pendeen Cove, Penzance Bay, England.

8. In the spring of 1882, the Swedish bark Abraham Lincoln picked up a buoy in lat. 32° 30′, long. 28° 40′.

9. October 22, 1883, a buoy was picked up on the east side of Teneriffe in lat. 28° 21′, long. 16° 15′.

10. October, 1883, a second buoy was picked up fifteen miles from the east coast of Teneriffe.

11. August 20, 1883, the British bark Jane Richardson picked up a buoy in lat. 24° 11′, long. 32° 43′.

All were identified as the property of the United States by letters cast in the plates.

The charted currents of the ocean readily explain the courses and account for the positions of many of these buoys, but others appear anomalous.

Mr. JENKINS cited an instance of a bell-buoy, carried away from the coast of the United States in 1850, which was seen and heard while adrift and finally stranded on the southwest coast of Ireland.

Mr. WELLING suggested that the phenomena might not be referable to ocean currents exclusively, but in part to wind currents. Mr. JOHNSON judged from the forms of the buoys that their movements would be controlled more by currents than by winds.

Mr. H. FARQUHAR and Mr. JENKINS were of opinion that the buoy picked up off Florida might have been carried there by the southward coast-current. Mr. DALL concurred, but thought it also possible that it had made the entire circuit of the Sargasso sea.

Mr. DALL, referring to Mr. WELLING'S suggestion, said that wind and current worked together, and their effects could not be discriminated. The wind does not blow prevailingly in any direction without coercing currents to correspondence.

247TH MEETING. FEBRUARY 16, 1884.

The President in the Chair.

Fifty-four members and guests present.

The Auditing Committee reported through its Chairman, Mr. C. A. WHITE, that it had examined the accounts of the Treasurer for 1883, finding the same properly vouched in respect to expenditures and receipts. On motion of Mr. DUTTON, the report was accepted.

The Chair announced the election to membership of Mr. HENRY WAYNE BLAIR and Mr. HERBERT GOUVERNEUR OGDEN.

Mr. F. W. CLARKE made a communication on

THE PERIODIC LAW OF CHEMICAL ELEMENTS.

After giving an account of the law as worked out by Newlands, Mendelejeff, and Lothar Meyer, he exhibited an enlarged copy of

Meyer's atomic volume curve, drawn with the latest values for both atomic weight and specific gravity. On the same sheet was also drawn a similar curve, illustrating the connection between atomic weight and melting point, and it was shown that in the latter the highest portions correspond to the lowest depressions in the atomic volume curve. The opinion was expressed, in view of the regularities exhibited by these curves, that the elements had originated by some method of evolution, and that a future transmutation of one element into another was not improbable.

In reply to a question by Mr. Farquhar, Mr. CLARKE said that search was being made for similar evidence of system in the spectra of the elements, but that the subject was rendered difficult by reason of the fact that not all the lines of the spectra fall within the range of visibility.

Mr. ANTISELL remarked that while the determination of the atomic weights of the elements was one of the most important labors which the modern chemist could be occupied with until a final constant numerical result should be arrived at, and until the other properties of matter which appear to have some definite relation with the atomic weight were rigidly investigated, there was necessity for continued effort to search into those hidden relations; but if by such investigation it was believed that we could arrive at any certainty about atoms, their form and structure, or about matter itself, we should be much disappointed. Situated as we are on a cold planet, we are precluded from ever arriving, by the study of matter from a standpoint merely terrestrial, at any ideas of the ultimate nature of atom or molecule, or whether there be really any such thing as "elements" or one form of matter wholly distinct from another. To arrive at a knowledge of matter, pure and simple, we must have ready means for dissociating all compound matter, and we have at our command at present no such methods or apparatus on this globe. Subjection to intense heat is required, and our most glowing furnaces and the arc light itself is insufficient for the purpose. It calls for the exhibition of such heat as is produced in the sun and its atmosphere to reduce our elements, as we term them, to the more simple condition of matter as it exists under solar temperature, and the present spectroscope and its future improvements by which such dissociation is to be studied. The

investigations of Huggins and Lockyer and other spectroscopists have revealed to us the presence of several of our so-called elements in the solar atmosphere; but constant observation has raised in the minds of these observers grave doubts whether the spectral lines of the elements, as obtained by observation of them in our atmosphere, are universally of such or whether only conditionally so, that is true only in our cold atmosphere. Doubts have arisen as to the spectral lines of elements being permanent characters of their essential nature, seeing that the spectral lines of an element, which at one time resemble those of copper, are found to be interchangeable and attached to a different element, as calcium, and that there are elements which possess the character of giving multiple spectra, as carbon, for example, which, under these solar temperatures, yields no less than three distinct and characteristic spectra.

In view of these apparently contradictory and confusing results, obtained by the examination of matter found in the solar atmosphere, which are so different from those obtained from matter in our own atmosphere, it behooves us to be very cautious in asserting the existence of any distinct elements so called, or whether there be only one matter under various cosmical conditions.

Other remarks were made by Messrs. Doolittle and White.

Mr. H. A. Hazen made a communication on

THE SUN-GLOWS,

opposing the theory that they are due to dust, either cosmic or volcanic, and advocating a theory involving electrical action in connection with frost particles.*

A general discussion followed, in which Messrs. Elliott, Paul, Robinson, Hall, Dutton, Gilbert, and E. Farquhar, participated.

Mr. Elliott advocated the electrical origin of the glows, basing his argument on the simultaneousness of the phenomena throughout the planet, on the transparency of the glow as shown by observations on Lyræ, and on the extraordinary abundance of sun spots for the past few weeks.

* This paper is published in full in the American Journal of Science for March, 1884; Vol. XXVII, pp. 201–212.

248TH MEETING. MARCH 1, 1884.

The President in the Chair.

Forty-two members present.

The Chair announced that Messrs. CHARLES OTIS BOUTELLE, GILBERT THOMPSON, WILLARD DRAKE JOHNSON, and EUGENE RICKSECKER had been elected to membership.

It was announced from the General Committee that standard time would hereafter be recognized in the opening and closing of the meetings.

Mr. R. D. MUSSEY read a paper entitled

THE APPLICATION OF PHYSICAL METHODS TO INTELLECTUAL SCIENCE.

The aim of the paper was to show in how far those methods which had been successfully employed in the investigation of the phenomena of nature, and which were denominated the Physical Sciences, were applicable to those sciences, the subject-matter of which were mental operations and their results, and which, for distinction, might be named the Intellectual Sciences. Some illustrations were given of the application of these methods to the study of the law; and the paper concluded with the remark that its writer desired it to be regarded as a suggestion rather than a solution of the problem stated: "How far and in what way physical methods and physical sciences help thinkers to say *Therefore.*"

Remarks were made by Mr. ROBINSON.

Mr. I. C. RUSSELL made a communication on

DEPOSITS OF VOLCANIC DUST IN THE GREAT BASIN.

[Abstract.]

In contrast with the aridity of the Great Basin at the present time, geologists have shown that during the Quaternary it was crowded with lakes. In studying the sedimentary deposits of one of these fossil lakes, named Lahontan by Mr. King, I found strata

of white, unconsolidated, dust-like material, which is undistinguishable in general appearance from pure diatomaceous earth. Beds of this material, varying in thickness from a fraction of an inch to four or five feet, were observed at a number of localities in the sides of the cañons that have been carved in lacustrine strata of Lahontan age by the Humboldt, Truckee, Carson, and Walker rivers. Deposits identical with those of the Lahontan sections were observed at a number of localities among the mountains of Nevada and California at an elevation of several hundred feet above the former level of Lake Lahontan and at a distance of forty or fifty miles from its borders, thus showing that the deposits were both sub-aerial and sub-aqueous in their mode of accumulation. Further exploration revealed the fact that similar beds occur abundantly in Mono Lake Valley, where they may be seen to pass into well-characterized fragmental deposits of pumice and obsidian, thus suggesting that the finer material was also of volcanic origin. Experiment confirmed this hypothesis. Under the microscope the dust from a number of widely separated localities was found to consist almost wholly of angular flakes of transparent glass, with scarcely a trace of crystallized matter. When a sample of pumice from near Mono Lake was reduced to a fine powder, it was found to present the same physical and optical properties as the dust in question, with which it also agreed closely in chemical composition, as shown by analyses made by Dr. Chatard, of the Geological Survey.

The Mono Craters, from which this dust is supposed to have been erupted, form a group of cones about fifteen miles in length, situated in the southeastern part of the Mono Lake Valley, California. These extinct volcanoes are composed almost entirely of pumice and obsidian, in the condition both of coulées and lapilli, the latter constituting cones of great symmetry and beauty, the grandest of which have an elevation of nearly three thousand feet above Mono Lake. Some of these craters were in eruption during Quaternary times, while others were active after the ancient lakes and glaciers of the region had passed away. Many times during their history vast quantities of lapilli and dust were thrown out. As the volcanic dust interstratified with the sediments of Lake Lahontan is undistinguishable from that deposited in the Mono Basin, there is little room for doubting that they had a common origin. The

greatest distance from the Mono Craters at which the dust was observed, was in the Humboldt Cañon, about two hundred miles northward of the point of eruption.

At three localities in the Lahontan Basin the bones of extinct mammals were found closely associated with the deposits described above, thus furnishing the suggestion that the showers of fine volcanic dust were, at least to some extent, fatal to animal life.

Mr. ANTISELL said it was useless to look for the source of volcanic dust in existing volcanoes on the land. Pumice in the character of fine particles, as exhibited, is exclusively the product of submarine eruption. Other remarks were made by Mr. HARKNESS.

Mr. LESTER F. WARD read a paper entitled

SOME PHYSICAL AND ECONOMIC FEATURES OF THE UPPER MISSOURI SYSTEM,

in which he described the process by which the valleys of the Lower Yellowstone and Upper Missouri are formed, and pointed out the importance and the feasibility of utilizing the water of these rivers for purposes of irrigation.*

Mr. GILBERT said that Mr. Ward's description of the process by which the Missouri constructs its flood plain was verified by a nearly identical group of phenomena observed by himself on the lower course of the Colorado. Mr. ELLIOTT concurred with the speaker's view that the system of irrigation should be inaugurated by national action rather than local. Mr. RILEY was of opinion that the proposed plan of irrigation was entirely feasible, and said that the final solution of the grasshopper problem lay in the cultivation of the northern plains.

Mr. BURCHARD said that while the political advantage of a continuous belt of settlement uniting the Atlantic and Pacific States was undeniable, he questioned the advisability of increasing at present our agricultural production.

* This paper was subsequently separated into its two natural divisions, and the part relating to the "physical features" was published with illustrations in the "Popular Science Monthly" for September, 1884 (Vol. XXV, pp. 594–605), while that relating to the "economic features" appeared in "Science" for August 29, 1884 (Vol. IV, pp. 166–168).

249TH MEETING. MARCH 15, 1884.

The President in the Chair.

Fifty members present.

The Chair announced the election to membership of Messrs. MARK BRICKELL KERR, SAMUEL HAYS KAUFMANN, JOSEPH SILAS DILLER, CHARLES HENRY WHITE, and WILLIAM LAWRENCE.

Mr. G. K. GILBERT made a communication on

THE DIVERSION OF WATER COURSES BY THE ROTATION OF THE EARTH.

[Abstract.]

It being admitted that the rivers of the northern hemisphere are, by the rotation of the earth, pressed against their right banks, and those of the southern hemisphere against their left banks, it remains to determine whether this pressure is quantitatively sufficient to appreciably modify the courses of rivers. Opinion is divided, and the results of observation have been largely negative. Those who regard the cause as insufficient to produce observable results have approached the subject from two points of view, which are illustrated by the discussions of Messrs. Bertrand and Buff. The former computes that a river flowing in N. lat. 45° with a velocity of three metres per second exerts a pressure on its right bank of $\frac{1}{63539}$ of its weight, and regards this pressure as too small for consideration. The latter points out that the deflecting force, by combining with gravitation, gives the stream's surface a slight inclination toward the left bank, thereby increasing the depth of water near the right bank, and consequently increasing the velocity of the current at the right. This increment of velocity has a certain erosive effect, but it is regarded as less than that assignable to wind waves on the same water surface, so that the prevailing winds have a more important influence than the rotation of the earth.

The object of the paper is to consider the theoretical effect from a new point of view. The form of cross-section of a stream flowing in a straight channel depends on the loading and unloading of detritus, and is essentially stable, its character being naturally

restored if accidentally or artificially modified. The distribution of velocities within this cross-section is symmetric, the swiftest threads of the current being in the center and the slowest adjacent to the banks. If now curvature be introduced in the course of the channel, centrifugal force is developed. This centrifugal force is measured by the square of the velocity, and is therefore much greater for the swift central threads of the current than for the slow lateral threads. The central threads, tending the more strongly toward the outer bank, displace the slower threads at that bank, and the symmetry of the distribution of velocities is thus destroyed. As pointed out by Thomson and others, this redistribution of velocities determines the erosion of the outer bank and the simultaneous deposition of detritus along the inner bank.

It has been shown by Ferrel that the deflecting power of the rotation of the earth upon a body moving on the surface is equivalent to the centrifugal force which would be developed if the body followed a circular course with radius of curvature (ρ) equal to $\frac{v}{2\, n \cos.\, \theta}$. In this expression v is the velocity of the body, n the angular velocity of the earth's rotation, and θ the polar distance of the locality.

The effect of rotation on a stream being equivalent to a centrifugal force is identical in kind with the effect of curvature of channel,* and this identity renders a quasi-quantitative comparison possible. Humphreys and Abbott found during flood a mean velocity of the Mississippi river at Columbus of 8.4 feet per second. The value of ρ corresponding to this velocity and the polar distance of the locality is about 20 miles. The actual bends of the channel in the same region, which depend for their features on the velocity and volume of the river at flood stage, have a radius of

* The author has since seen reason to modify this statement. The two effects are not strictly identical in kind, because the effect of rotation varies with the first power of the velocity, while the effect of curvature of channel varies with the second power. For this reason the selective power of curvature is, for the same deflective force, double the selective power of rotation. The introduction of this consideration would modify the numerical results derived from the Mississippi river, but would not impair the qualitative conclusion. A modified treatment of the subject will be found in the American Journal of Science for June, 1884; Vol. XXVII, pp. 427–432.

curvature of about 1½ miles. Centrifugal force being a simple inverse function of radius of curvature, it follows that the deflective force by which the river is impelled toward its right bank by virtue of rotation is proportioned to the force by which it is impelled toward its outer bank on acute bends in the ratio of 1½ to 20. That is to say, in this particular instance the rotational deflective force is 7½ per cent. of the deflective force from curvature of channel.

The process of lateral corrasion is so complex that it is impossible to convert this result into terms of erosion and consequent deflection of stream channel, but a consideration of the manner in which the two deflective forces are combined sufficiently indicates that that due to rotation cannot be ignored. Wherever the stream bends toward the left the centrifugal force developed by the curvature is augmented by the rotational force; wherever the stream turns toward the right the centrifugal force is diminished by the amount of the rotational force; so that the tendency of the swiftest threads of current to approach the outer bank must be notably greater in one set of bends than in the other.

If this analysis of the subject is legitimate, the rotation of the earth ought surely to modify the courses of rivers to such extent that the modifications are observable phenomena. Exception should however be made of two important cases: first, rivers which are rapidly deepening their channels are by that fact held rigidly to their original courses, and are not deflected either by rotation or by any other cause; second, those parts of rivers whose function is deposition instead of erosion, should theoretically, under the influence of rotation, built their alluvial plains higher on the right hand side than on the left, and having established an inclination of the alluvial plain toward the left, should thereafter meander over the plain with equal facility in all directions. It is only in the middle courses of streams, where the work performed by the water is chiefly that of transportation, that the discovery of the effects of rotation should be expected.

Mr. WARD remarked that in the regions especially discussed the river courses are, in general, southerly, while the prevailing winds are westerly, so that the influence of the winds is opposed to whatever influence may be exerted by rotation. Mr. ABBE said that the tendency of driftwood toward certain river banks, cited by

von Baer, had been plausibly explained as due to prevailing winds, but such action is purely or chiefly superficial, and a less important factor in erosion than the behavior of the main current, which is comparatively little influenced by winds. Nevertheless, he was surprised that the rotational influence admitted of so large a quantitative expression.

Mr. DALL said that the northward-flowing rivers entering the Arctic ocean afforded at their mouths no evidence of the effect of rotation. The summer winds of Arctic regions are from the northeast and east, and these produce on the north coast of America a shore-current, which drifts the beach sand and shingle westward, and deflects the river-mouths in the same direction. All the rivers from the Mackenzie to Point Barrow illustrate this tendency. On the coast of Siberia the fresh water discharged by the large rivers has been observed to turn eastward, although the winds would tend to throw it the opposite way. The Arctic ocean is there deeper; and it is believed that its principal currents are controlled by the northeasterly set of the general currents of the North Atlantic.

Mr. ROBINSON spoke of the indirect influence of wind on river channels, through drifting sand. Mr. HAZEN pointed out that the influence of wind might be eliminated from the problem by studying the streams running east or west. Mr. BOUTELLE suggested that the course of the Mississippi did not indicate any result of rotational influence. Mr. E. FARQUHAR inquired whether the behavior of the Gulf Stream and other ocean currents was in accordance with the theory of rotational influence; and Mr. DALL responded that in the discussion of ocean currents this cause had lately dropped out of sight, the determination of courses being ascribed to the winds.

Mr. MUSSEY inquired whether the acuteness of continental masses toward the south admitted of an explanation based on the effect of terrestrial rotation; and Mr. DUTTON responded by saying that the mass of speculation in regard to the recurrence of certain forms of continental outline had never really accomplished more than the statement of the fact. The fact itself is an accident, dependent on the volume of the ocean and the general laws governing the formation of mountain chains. If the ocean were five hundred feet deeper, or five hundred feet shallower, the forms of

continents would be so far different that all the existing resemblances would disappear. The pointed extremities of some continents are merely expressions of the fact that mountain chains are more or less linear, and do not hold the same height throughout their whole extent.

Mr. G. E. CURTIS read a paper on

THE RELATIONS BETWEEN NORTHERS AND MAGNETIC DISTURBANCES AT HAVANA,

upon which remarks were made by Messrs. ABBE and COFFIN. [It will be published by the Army Signal Office as *Signal Service Note No. XIII.*]

Mr. GILBERT recurred to the subject of Mr. Russell's paper of the preceding meeting, and dissented from the view advanced by Mr. Antisell in regard to the origin of pumice. Mr. ANTISELL announced that he would discuss the matter more fully at some future meeting.

250TH MEETING. MARCH 29, 1884.

Vice-President MALLERY in the Chair.

Forty-two members present.

The Chair announced the election to membership of Messrs. BASIL NORRIS and WILLIAM STEBBINS BARNARD.

Mr. J. S. BILLINGS spoke briefly on

COMPOSITE PHOTOGRAPHY APPLIED TO CRANIOLOGY,

exhibiting several composite photographs of skulls. Adult male skulls of the same race were selected for composition and were photographed in sets of from 7 to 18—front, side, and back views being separately taken. The composition was directly from the skulls and not from the photographs.

Incidental mention was made of the uncertainty of measurements of cranial capacity by means of shot. Not only did differ-

ent observers obtain widely different determinations from the same skull, but the same observer was not able to obtain closely approximate results in successive determinations.

Mr. G. BROWN GOODE made a communication on

FISHERIES EXHIBITIONS,

giving a list of all international exhibitions and describing especially those of Berlin (1880) and London (1883). The administrative systems of these two national exhibits were contrasted, and the social and economic results of the London exhibit were explained. [The substance of the paper will be published in the executive report on the London and Berlin exhibitions.]

Mr. M. H. DOOLITTLE began a communication on

MUSIC AND THE CHEMICAL ELEMENTS,

but was unable to complete it before the hour for adjournment. The remaining portion was postponed until the next meeting.

By unanimous consent adjournment was deferred for a few minutes in order to afford Mr. Antisell an opportunity to reply to a criticism made at the previous meeting in regard to his views on the origin of pumice.

251ST MEETING. APRIL 12, 1884.

The President in the Chair.

Forty-one members and guests present.

Announcement was made of the election to membership of JAMES ARRAN MAHER, JOHN BELKNAP MARCOU, JOHN MILTON GREGORY, FRANCIS TIFFANY BOWLES, and WILLIAM EIMBECK.

Mr. M. H. DOOLITTLE made a communication on

MUSIC AND THE CHEMICAL ELEMENTS.

[Abstract.]

The mathematical theory of music requires the satisfaction of the equation $2^x = \left(\frac{3}{2}\right)^y$ *nearly;* in which, for equal temperament, x = the number of equal intervals in the octave, and y = the number of these intervals that correspond to a nearly perfect fifth; and, for untempered music, x = the number of approximately equal intervals in the octave, and y = the number corresponding to a perfect fifth.

The above equation gives

$$\frac{x}{y} = \frac{\log \frac{3}{2}}{\log 2} \text{ nearly} = \frac{176091}{301030} \text{ nearly;}$$

and by the method of continued fractions we obtain the succession of approximations $\frac{3}{5}$, $\frac{7}{12}$, $\frac{24}{41}$, $\frac{31}{53}$, &c.

For scales appropriate to major thirds, but disregarding fifths, we may substitute $\frac{5}{4}$ for $\frac{3}{2}$ in the above equations, and obtain the approximations $\frac{1}{3}$, $\frac{9}{28}$, $\frac{19}{59}$, &c. For the chord having the vibration ratio 7 : 4 (called by Ellis the subminor seventh), we may obtain in like manner the approximations $\frac{4}{5}$, $\frac{21}{26}$, &c.

Since $\frac{1}{3} = \frac{4}{12}$, the first two series of approximate fractions include a common scale of twelve intervals to the octave, of which seven intervals give the fifth, and four give the major third. The first and the third of these series include a scale of five intervals to the octave, of which three constitute the major third, and four constitute the subminor seventh. There is some reason to believe that this is the scale of Japanese music, with the intervals $\frac{7}{6}$, $\frac{8}{7}$, $\frac{9}{8}$, $\frac{7}{6}$, $\frac{8}{7}$. Five-tone scales have universally prevailed in early music; but it is questionable whether the vibration ratios

have in any case involved the prime number seven. It would be interesting to know what scale best represents the songs of wild birds.

There is much reason to believe that simple mathematical principles underlie the phenomena of chemistry. It is not, *à priori*, absurd to suppose that matter in some way conforms to the properties of the primes 2, 3, and 5; in which case such derivative numbers might be expected prominently to appear as prominently occur in the science of music. The fraction $\frac{7}{12}$ might reasonably be expected.

If all the keys of a piano should be arranged seven consecutive keys in a line, the next seven in the next line, and so on, the columns give successions of fifths. It has been shown that if the chemical elements are arranged in the order of their atomic weights in lines of seven, the columns contain elements remarkably similar to each other. We seem to have a chemical scale remarkably analogous to the ordinary musical scale. If the piano keys be arranged in lines of twelve, the columns give octaves; but nothing is developed from a similar arrangement of the chemical elements, whence it may be inferred that the observed analogies are accidental, and have no true logical basis.

If the intervals of the chemical scale could be supposed to correspond to the seven intervals of the diatonic scale, the non-appearance of the twelve-fold relation would be accounted for; but, while the diatonic scale may have some claim to be called natural, it is not directly established by algebraic investigation of the relations of prime numbers. Until the discovery of chemical flats and sharps, there will be insufficient reason to regard the present chemical scale as diatonic.

Mr. LEFAVOUR illustrated the connection between tone and wave-length by means of a logarithmic spiral of base 2, the harmonic notes having *radii vectores* equal to multiples of the principal note.

Mr. ELLIOTT said he had learned from Mr. Poole that he had endeavored, in his euharmonic organ, to produce perfect chords in all keys without temperament.

Mr. KUMMELL remarked that in modern music the intervals of the major and minor thirds are the most important, because with-

out them there is no harmony. This is also apparent from the well-known rule in thorough-bass that a third with its fundamental note is to be treated as a complete chord. Now it happens, in dividing the octave by equal temperament into 12 equal parts, that a major third is nearly 4 and the minor third nearly 3 of these, and thus we obtain not only tolerable fifths, but also tolerable thirds, and the requirement of thirds for harmony is approximately fulfilled. They are still better fulfilled, of course, if we divide the octave into 41 or 53 parts, as Mr. Doolittle has shown. As to the seventh harmonic, Poole and Helmholtz rightly hold that it should be and is used by instruments which can temper. It is obviously the fourth element of the chord of the dominant G, B, D, F, the F being the seventh harmonic to the G two octaves below (nearly so in equal temperament and exactly in natural harmony), and this chord in modern music forms the opposing harmony to the tonic chord C, E, G, in major, and C, E flat, G, in minor. Instruments with fixed tones like the piano-forte have to use equal temperament, and thus virtually reject all natural harmony except the octave. This defect is generally inappreciable in very slow movements, but may be noticed by a very cultivated ear.

Other remarks were made by Messrs. CLARKE, MUSSEY, and HARKNESS.

Mr. H. FARQUHAR read a

REVIEW OF THE THEORETICAL DISCUSSION IN PROF. P. G. TAIT'S "ENCYCLOPÆDIA BRITANNICA" ARTICLE ON MECHANICS.

[Abstract.]

This article covers seventy-four quarto pages in the last edition of the Encyclopædia, and gives a thorough mathematical treatment of the subject. No innovations calling for comment—unless an extended use of the "fluxional" notation for derivative functions be so regarded—appear until near the end, where two and a half pages are devoted to a disproof of the objective reality of force, and an advocacy of the disuse of the term in scientific writing. The character of the publication, and the eminence of the author in mathematics and physics, entitle his arguments to a careful examination.

In the first place, Prof. Tait infers that force can have no such reality as matter has, because it is to be reckoned positively and negatively—an action being opposed by reaction—while matter or mass is signless. This suggests two comments: (1), the author never questions the objective reality of space and time, of which realities it is an essential feature that to every direction or interval A–B, an equal direction or interval B–A, of opposite sign, corresponds; (2), the idea of a negative mass is not a self-contradictory one, and was once generally accepted. The element phlogiston was given up not because of any absurdity in ascribing levity to material substance, but because a form of matter with positive mass (oxygen), capable of explaining all the phenomena, had been actually separated and identified.

Prof. Tait's next criterion of objective reality is quantitative indestructibility, an attribute shared by time, space and matter, to which he adds energy. But the evidence of the indestructibility of energy is not of the same nature as that of the indestructibility of matter; for the latter in all its forms may be localized, and its density or elasticity measured; while the former, when stored up or "potential," cannot be shown to possess any of the properties of energy kinetic, or any existence in space, or any objective character whatever. Prof. Tait admits this difficulty virtually, and awaits for its solution the discovery of some evidence "as yet unexplained, or rather unimagined." All strains and other actions of a clock-weight on its supports are obviously precisely the same—or impalpably somewhat stronger—with the weight wound up an inch, as with it wound up a yard; and the existence of a greater "potential energy" in the latter case is to be found not in the clock, but in the mind, which requires this expression as a form in which to put its conviction that a certain greater amount of work can be obtained. Even though it be admitted that there are no other intelligible terms in which this conviction can be stated, it is clear that the indestructibility of energy is an ideal and subjective truth, and cannot, therefore, be relied on as evidence of a reality distinctively "objective."

A third point made by Prof. Tait against force is that its numerical expression is that of two ratios: "the space-rate of the transformation of energy" and "the time-rate of the generation of momentum." These results are obtained by simple division, in an

equation which expresses the fact that the work done by a body in falling the distance h is just that required to lift it through h against gravity. The fallacy involved in treating the numerical expression for force as force itself, has been well exposed by Mr. W. R. Browne (in a criticism of the same article, *L. E. D. Phil. Mag.* for November, 1883); and the assumption that ratios are necessarily non-existent is even more fallacious. Were it trustworthy, Prof. Tait's deductions would not be the only ones admissible. His equations would lead quite as conclusively to proofs of the non-objectivity of space and time (the former becoming the rate of work-units, the latter of motion-units, per unit of force), and so to a confirmation of the celebrated German view, that that which is universal and necessary in thought, belongs to the Subject; or they might even give mass in the form of a ratio, and hence suggest the non-objectivity of matter.

Not the least of the Professor's objections against force, it would appear, is that it is "sense-suggested." It is a mere truism to say that no other suggestor is possible, within the domain of science. It is, perhaps, better worth while to call attention to the indubitable fact that the real, if not the avowed, ground of the objection against "action at a distance," entertained by many physicists, is that such action is not directly suggested by sense-impressions. This is what they must mean by calling it "occult;" actions as our consciousness knows them, and as we can produce them, being generally characterized by proximity undistinguishable from actual contact. Further, if there is any reproach in this epithet, energy is quite as open to it as any function of energy can be. In fact, our senses directly report work, in the form of nerve-disturbance, and nothing else. Force is no more truly an inference from nerve-reports testifying of energy exerted, than is matter. In fact, the inference of the independent existence of matter is the less direct and more questionable of the two. The view advocated by Mr. Browne, following Boscovich, that matter is but "an assemblage of central forces, which vary with distance and not with time" or with direction, is one of great simplicity as well as suitability to analytic treatment, and one of which no disproof is possible.

The paper was discussed by Messrs. DOOLITTLE and ELLIOTT.

252D MEETING. APRIL 26, 1884.

Mr. HARKNESS in the Chair.

Thirty-eight members and guests present.

Announcement was made of the election to membership of Messrs. DAVID PORTER HEAP and THOMAS MAYHEW WOODRUFF.

Mr. J. R. EASTMAN made a communication on

A NEW METEORITE.

[Abstract.]

A mass of meteoric iron weighing 113 pounds was acceccidently discovered in the making of an excavation at Grand Rapids, Michigan, and was examined by the speaker in 1883. One face shows evidence of fracture, and the greater part of the remaining surface, of fusion. A very small sample submitted to Mr. F. W. Taylor for chemical examination had a specific gravity of 7.53 and a composition:

Iron	94.54
Nickel	3.81
Cobalt	.40
Insoluble (about) . .	.12

The stone is supposed by its holders to consist of gold and silver, and to be the buried treasure of a miser. This delusion has caused it to form the subject of a lawsuit.

The communication was discussed by Messrs. BATES and F. W. CLARKE.

Mr. W. H. DALL read a paper on

CERTAIN APPENDAGES OF THE MOLLUSCA.*

* Published in the American Naturalist, Vol. XVIII, pp. 776–778.

Mr. J. S. Diller made a communication on

THE VOLCANIC SAND WHICH FELL AT UNALASHKA OCTOBER 20, 1883, AND SOME CONSIDERATIONS CONCERNING ITS COMPOSITION.

[Abstract.]

The sand is composed chiefly of crystal fragments of feldspar, augite, hornblende, and magnetite, with a considerable proportion of microlitic groundmass and a very few splinters of volcanic glass. Its mineralogical composition is that of a hornblende andesite; but the chemical analysis by Mr. Chatard shows it to contain only 52.48 per cent. of silica,—which is much more basic than the average for that group. The character of the minerals, as well as the general composition of the sand, indicated so clearly that the crater from which it must have issued was erupting hornblende-andesite, that I was led to seek an explanation for its paucity in silica.

With this purpose in view, a number of volcanic sands and dusts from various parts of the world were examined and compared with the lavas to which they belong. First and most important among these is a sand from Shastina, a crater named by Captain Dutton, upon the northwestern flank of Mt. Shasta, in northern California. This sand, like that from Unalashka, is composed chiefly of crystal fragments of feldspar, augite, hornblende, and magnetite, with fragments of microlitic groundmass. Besides these, there are many pieces of hypersthene crystals and pumiceous glass. The sand contains 60.92 per cent. of silica, while the hornblende-andesite lava (rich in hypersthene) of Shastina, to which the sand belongs, contains 64.10 per cent. of silica.

From these and other examples it may be stated as generally true that volcanic sand is composed essentially of crystalline fragments, and contains less silica than the lava to which it belongs.

With volcanic dust, however, the case is different. Microscopical examination shows that it is composed chiefly of volcanic glass particles; and as far as chemical analyses have been made, they indicate that volcanic dust is more silicious than the lava to which it belongs.

That volcanic sand should be crystalline and basic, and the accompanying dust vitreous and acidic, as compared with the lava

to which they belong, is not merely determined by accidental circumstances, but has its inception in the magma before the eruption takes place. By the process of crystallization magmas are frequently divided into a crystalline solid portion, and an amorphous more or less fluent portion. Basic minerals are the first to crystallize, so that as the process advances the amorphous remnant of the magma becomes more and more silicious. The crystals are generally thoroughly intermingled with the amorphous magma, and in the latter are accumulated nearly all of the absorbed gases under great tension, so that when the pressure is relieved it may be blown to fine silicious dust, which may be carried by the wind many miles from its source, while the solid crystalline portion will contribute chiefly to the formation of sand, and be precipitated comparatively near the crater from which it issued.

In cases where no previous crystallization has taken place in the magma before it comes to violent eruption, the volcanic dust then formed will have about the same chemical composition as the lava to which it belongs. Mr. Russell has recently described an interesting case of this kind in the western part of the Great Basin. It appears to be generally true that if other conditions are favorable the difference in chemical composition between volcanic sand and dust is directly proportional to the amount of crystallization in the magma before its ejection.

The basic character of the Unalashka sand may be explained by supposing that the silicious portion of the magma was carried away in the form of dust.

The source of this sand is supposed by the collector, Mr. Applegate, the Signal Service Observer at Unalashka, to have been the new crater formed last autumn, near the Island of Bogosloff, about sixty miles away.

Mr. Dutton spoke in commendation and amplification of Mr. Diller's contribution to geologic philosophy. Mr. Dall described the geographic relations of the volcano from which the Unalashkan dust was presumably derived, showing the improbability of the eruption having been directly observed. He spoke also of the distribution of the Aleutian volcanoes and the lithologic characters of their *ejectamenta*.

There ensued a general discussion of the nature and properties

of volcanic dust and of the theory which ascribes recent meteorologic phenomena to the dust ejected by Krakatoa. In this Messrs. DUTTON, PAUL, W. B. TAYLOR, DILLER, ROBINSON, and WARD participated. Mr. DUTTON pointed out that their process of formation tends to give volcanic dust particles a quasi-definite size, and probably does not produce a large amount of dust fine enough for indefinite suspension. The greatest distance to which volcanic dust has been definitely ascertained to travel is eight hundred miles.

Mr. PAUL argued from the violence of the Krakatoan explosion its competence to charge the atmosphere at very great altitudes, and considered the fineness of the dust a sufficient explanation of its indefinite suspension.

Mr. TAYLOR said the phenomenon to be accounted for was specially remarkable, first, for the unusual elevation of the finely-divided smoke or dust extending far above the highest cirrus clouds, or probably to twenty or thirty miles above the earth's surface (as shown by its twilight duration); secondly, for its wide diffusion (covering a large fraction of the terrestrial atmosphere); and thirdly, for the long continuance of its suspension in the air (extending over many months). Mr. Lockyer and Mr. Preece had suggested an electrical condition of the matter as favoring both its extraordinary diffusion and its equally extraordinary suspension. This hypothesis seemed to the speaker very plausible. Electricity is a phenomenon of volcanic eruption, and dust particles charged with electricity in the same sense with the earth would be repelled not only by one another, but by the earth. At thirty miles above the ground the air is not only very rare, but is practically anhydrous, and the discharge of electricity is impossible.

Mr. DILLER, in response to a question by Mr. Paul, said that the microscope reveals no limit to the fineness of Krakatoan dust. The higher the magnifying power applied, the greater the number of particles visible; and this relation extends to the limits afforded by the capacity of the instrument. To more powerful microscopes, yet finer particles would presumably be visible.

253D MEETING. MAY 10, 1884.

The President in the Chair.

Fifty-four members and guests present.

Announcement was made of the election to membership of Messrs. JOHN MURDOCH, ROMYN HITCHCOCK, WILLIAM SMITH YEATES, GEORGE PERKINS MERRILL, and FREDERIC PERKINS DEWEY.

It was announced that a vacancy in the General Committee, occasioned by the resignation of Mr. J. J. KNOX, had been filled by the election of Mr. F. W. CLARKE.

By invitation, Mr. G. H. WILLIAMS, of Baltimore, Maryland, addressed the Society on

THE METHODS OF MODERN PETROGRAPHY,

first, defining the field of petrography, and second, discussing the methods of petrographic investigation. These methods are: (1), chemical; (2), mechanical; (3), optical; (4), thermal. The chemical methods are quantitative and qualitative. The mechanical methods include the separation of the constituent minerals of rocks by precipitation in heavy solutions and by the use of electro-magnets. The optical methods include the preparation of thin sections, their examination by transmitted ordinary light, and their examination by polarized light, for the determination of crystallographic system, pleochroism, and angles of extinction. The thermal methods are chiefly synthetic, consisting in the artificial production of mineral aggregates for the purpose of determining the processes of their natural production. By the regulation of temperatures in fusion and refrigeration all varieties and all structures of basic rocks are reproduced. Acidic rocks have not been thus reproduced, and it is believed that great pressure is a condition of their genesis.

Mr. DUTTON spoke of the bearing of modern petrographic investigations on some of the greater problems of geology.

There followed a symposium on the question

WHAT IS A GLACIER?

[Abstract.]

Mr. I. C. Russell: In framing a definition of a glacier it is evident that we must include both alpine and continental types, and also take account of the secondary phenomena that are commonly present. With this preamble we may define a glacier as an ice-body, originating from the consolidation of snow in regions where the secular accumulation exceeds the loss by melting and evaporation, *i. e.*, above the snow-line, and flowing to regions where loss exceeds supply, *i. e.*, below the snow-line.

Accompanying these primary conditions, many secondary phenomena dependent upon environment, as crevasses, moraines, lamination, dirt-bands, glacier-tables, ice-pyramids, etc., may or may not be present.

Mr. S. F. Emmons: The glacier is a river of ice, possessed, like the aqueous river, of movement and of plasticity. In virtue of the latter quality it adapts itself, though more slowly, to the form of the bed in which it flows. The névé field is the reservoir, from which it derives not only its supply of ice, but the impulse which gives it its first movement. The névé is formed by the snows which accumulate in relatively wide basins above the snow-line from year to year, living through the heat of summer. Its mass may be more or less compact, according as it is thicker or thinner, and it may have a certain movement, which will be greater or less, according to the greater or less inclination of the basin; but until it moves from its wide and shallow bed into a narrower and deeper one, and thus gives outward proof of the plasticity of the ice of which it is composed, it does not become a glacier. It may be crevassed. Often a long crevasse at its upper edge gives definite proof of its movement; and this movement may cause a cracking or crevassing in other points, resulting from the unevenness of its bed. It may or may not carry blocks of rock on its surface, but these would be rare, and never in the well-defined moraine ridges that are formed upon the glacier proper. Not, however, until its form had essentially changed to fit the bed in which it flows should it be considered to constitute a glacier proper.

Mr. W J McGee: The phenomena of glacier ice and névé ice appear to belong to a graduating series; and in consequence the two phases can only be arbitrarily discriminated. Any classification depending upon coincidence of the *loci* of apparent transition from the first phase to the second with *loci* of sudden constriction or abrupt acclivity in the valley is artificial and incompetent, since such coincidence is fortuitous; the classification depending upon the ability of the second phase to sustain bowlders upon its surface is superficial and incompetent (provided such ability be due to density of the ice), since the sub-surface density of the névé, being determined by its age and the pressure of the superincumbent mass, must, in some portions, equal the surface density of the glacier; and the classification depending upon rate of motion is equally incompetent, since motion is common to the entire ice-mass, and abruptly varies only where conditions of glacier-bed are suddenly variant. Arbitrary diagnostic characters may and should be, however, agreed upon by consense among glacialists. Perhaps the most satisfactory line of demarkation detectable is the *snow-line*, above which superficial débris is buried by precipitation, and below which it is exposed by ablation.

Mr. W. H. Dall: It is proper to discriminate masses of ice moving in a definite direction from the immense fields of ice which are practically stationary. The term "glacier" should be restricted to the former. A glacier is a mass of ice with definite lateral limits, with motion in a definite direction, and originating from the compacting of snow by pressure. Moraines are not diagnostic; and the definition should not include those masses of arctic ice which, by reason of their low temperature, are fixed in position.

Mr. T. C. Chamberlin: Nomenclature is a matter of convenience. When subjects rise into familiar thought and frequent reference brevity of expression calls for specific names. But terms arising thus from a natural demand are not closely discriminative. Hard and fast lines of demarcation do not prevail in nature, but rather gradations of character. Were it otherwise names of sharply-defined application could be more freely used. The terms *névé* and *glacier* doubtless originated to satisfy the convenience of guides and travelers, and were without strict scientific application. In attempting to give them scientific definition, I think we shall fail of satisfaction by making them structural terms. The better distinction

is genetic. There is an area of growth and an area of waste to every glacier, and the distinct recognition of the two in quaternary glaciers is likely to rise to some importance. Superficially the area of growth coincides with the névé; the area of waste is that of the glacier proper. From every annual snow-fall there remains, at the time of maximum summer melting, a remnant that feeds the glacier. This is the névé for that year. The area may be greater or less in different seasons. The névé-field is accurately shown only on the day of maximum waste.

A contribution of much value, bearing upon the property of ice which permits glacier motion, has recently been made by Petterson, who has demonstrated, by refined experimentation, that ice, especially if impure, *shrinks* as it approaches the melting point and becomes plastic.

Mr. C. E. DUTTON desired to reiterate the remarks of Mr. Chamberlin to the effect that definitions can rarely or never be made rigorous. Glaciers, no doubt, vary in their characteristics like almost all other groups of phenomena. There is little difficulty in recognizing a glacier when all those features which characterize it are present, and when the conditions are of the ordinary nature. But exceptional cases arise. The lower parts are sometimes wanting and the névé alone remains, or the portion where the névé passes into the glacial stream may constitute the termination. In the latter case those who desire to be extremely precise in their phraseology might hesitate. It should seem best, whenever an occurrence is modified or defective, to use the term "glacier," with a qualification which shall express the particular circumstancs.

Remarks were also made by Messrs. GILBERT and ELLIOTT.

254TH MEETING. MAY 24, 1884.

The President in the Chair.

Twenty-six members and guests present.

It was announced from the General Committee that after the 255th meeting, June 7, the Society would take a vacation until October 11.

A request on behalf of the coming Electrical Exhibition at Philadelphia for instruments and books was communicated to the Society.

Mr. H. H. BATES read the following paper on

THE PHYSICAL BASIS OF PHENOMENA.

If there is anything entirely disheartening, it is to see the few landmarks of human achievement disappear before the shifting current of opinion, as headlands disappear under the ceaseless buffeting of the ocean. It is no doubt a matter of poignant regret to the cherisher of ardent theological convictions to see the bulwarks of faith slowly undermined by controversy. So, also, to him who has built his convictions on supposed demonstrable and irrefragable fact, to find nothing unassailable, not even the axioms and postulates conceded for ages as first principles, on which the fabric of science was reared, nor the sublime inductions of Galileo and Newton, on which the modern philosophy called natural—the only fruitful philosophy which man has produced—has been founded.

But the course of criticism shows that there are no first principles. Nothing is unquestionable. Even the mathematic joins hands with the metaphysic. I propose briefly to examine the fundamental grounds of mechanical philosophy, in view of the wide divergence of basal hypotheses in recent years, and especially on account of the importance conferred upon certain speculations by their admission into works of standard reference and authority.*

To do this aright it is necessary to go behind the mere sub-science of mechanics to the essence and substance of things, as did the eighteenth-century philosophers succeeding Newton. The observational data which have accumulated since that time by the splendid efforts of the molecular physicists enable us to review and recast, with some promise, the primary dogmas regarding the physical basis of phenomena. It is legitimate to frame hypotheses on subjects which are still unfathomed, but which confessedly do not belong to the domain of the unknowable. The distinguished example of the authors of the vortex atom would alone justify such a conclusion.

No entirely satisfactory hypothesis of the atom has yet been

* Encyclopædia Britannica, 9th Ed., Articles "Mechanics," "Measurement," etc.

found. I do not design to discuss the vortex atom here at length; for, although it is the most successful form of the Cartesian doctrine of vortical substance, it has not been perfected, and is generally regarded rather as an example of remarkable speculative and mathematical ingenuity, than as a discovery, corresponding with any facts of objective physics. It has insuperable difficulties, some of which have been pointed out by Clifford, and others by Clerk-Maxwell. Moreover, unparticled or continuous substance, the necessary postulate in this hypothesis, is something we not only have no experience of, but find full of inconsistencies with experience, when we gain a clear conception of what it implies. Such a conception fulfills Hegel's paradox that being and non-being are the same, since it forbids all mobility, all differentiation, as was perceived by the followers of Democritus. It simply affords an inviting basis for analytical discussion, on account of the elimination of the very conditions of objective existence which make the mathematical difficulty.

There are some postulates regarding substance which we may probably be permitted to assume at the outset. We may postulate its objectivity, and also its discontinuity. I have no space to review here the time-worn controversy between continuous and discontinuous substance. The arguments, which are exhaustive from the metaphysical side, are as old at least as Democritus and Anaxagoras. Suffice it to say that modern experiential philosophy has decided the battle experimentally in favor of the discontinuity of matter. The dispute only lingers in the region of the atom, where observation cannot penetrate or has not penetrated. The inability to conceive which attaches to all non-experiential affairs is encountered here, coupled with the too great facility of conceiving what is superficially observed, but will not bear analysis. Thus our first impressions of substance are in favor of its continuity. It is only after much reflection that we get the idea of necessary discontinuity, as bound up with the exhibition of existing phenomena. But the wonderful development of the Cartesian mathematics, in conjunction with the infinitesimal calculus, and its great facility in dealing with geometrical continuities, has tacitly revived the Cartesian idea regarding the nature of matter, as synonymous with space relations, which never reached intelligible development at the hands of its author, and wholly declined and disappeared after the

establishment of the Newtonian philosophy, and the discovery of the discrete character of substance.

In point of fact, experience would point to extreme porosity or discreteness as characteristic of substance, rather than to its opposite—perfect continuity. The infinite divisibility of space has nothing in the world to do with the question, though this is a confusion often fallen into. On the contrary, there is an infinite distinction between the infinitesimal discrete units of substance, occupying extension by their interactivity, and the passive infinitesimal resolvability of space continuity. This is the antipodean difference between the Epicurean and the Cartesian conceptions; the former admitting of the operations of force, the free exhibition of motion, the organization of material phenomena, which are phenomena of mobility; the latter constituting a plenum, with only ideal divisions, and phenomenally as necessarily barren a negation as space itself.

Substance is purely experiential. In its essence it is still incomprehensible, because experience has not yet reached down to those recesses. We know nothing of substance except by its manifestations. These manifestations are cognized by us through sense impressions, weighed, compared, adjusted, and analyzed in the mysterious alembic of the mind. First impressions have enormous predominance, and are intensified by heredity of cerebral predisposition and function.

We cognize substance only in bulk by direct perception, and these vast aggregations stand in thought for matter. A drop of water contains incomparably more molecules than the ocean contains drops; a grain of sand more particles than the earth contains grains; and it is this vast mesh of complicated forces that forms the integrated concept of matter to our apprehension. The child, before he can walk, encounters obstacles to movement, reaction to his every muscular effort, of equal measure to his own; and thus his first and profoundest convictions of objective existence are associated with resistance, opposition, repulsion. This impression of matter is so early that it remains with us as its most natural and obvious characteristic.

The idea of weight is also one of the earliest experiences. This idea would not be conceivable to a denizen of the deep sea, for our first ancestor who emerged from the water gained the experience at the cost of great struggle and enterprise. By the natural devel-

opment of muscle and function the child rears itself very early against the constant pull of our pedestal, triumphs over it with new-found energies, dances on tiptoe, and spurns the ground, but is soon content to draw the battle, to wander around a few weary years on equal terms, at length to call in the aid of a stick or crutch, and, finally, to resign the unequal contest, and sink, vanquished and satisfied, to rest in its bosom. Weight thus seemed a natural characteristic of matter until identified and generalized by Newton as a universal and especially a reciprocal property. This generalization transferred the property, in conception, from the naturally heavy body to a cause outside thereof, namely, the earth itself. Here the human mind relucted, for, unlike repulsion, attraction is not an observational fact. All forms of tension, stress, constraint—by whatever name called—are attended in the child's experience with an intermediary connection. The string is necessary to pull the cart, and the action of the magnet upon the iron particles is viewed with astonishment and awe. The sense of mystery does not proceed so far in his case as to contemplate the equally mysterious power which makes his string differ from a rope of sand. The most profound attention of the human mind has not yet fathomed this mystery.

Inertia or mass is a less obvious property, being in early observation and in common apprehension bound up with weight. It was not recognized in philosophy till Galileo's time, nor is it now by the common perception, except after training. A lady makes no scruple of asking to have a loaded car or train or vessel stopped at a given point on the instant, and reinvested with motion any number of times; and would-be inventors often contrive theoretical machines having numerous heavy reciprocating parts timed to velocities impossible of execution. With beings under other conditions it is wholly different. The sword-fish, *e. g.*, can have no conception of gravity, as he has no perception of it, but his apprehension of inertia is finely cultivated, through the muscular sense, in setting up and modifying the rapid movements in which his existence delights, as well as through his vivid realization of momentum, in the piercing of a whale or a vessel, by which his function is so powerfully exhibited. When once realized by human perception, however, inertia becomes identified with substance as its most primary characteristic.

The old scholastic property of impenetrability, also, is one of the superficial notions of experience, gained in the same way as that of repulsion. It seems to pertain to solids—the typical matter—with approximate accuracy, though calcined plaster of Paris and water, *e. g.*, will occupy a good share of each other's volume, and still form a highly porous solid. But a quart receiver full of hydrogen can have a quart of carbonic acid gas deftly introduced into it as into a void space; and so can a quart of water, at ordinary temperature and pressure, according to Gmelin, without increase of volume, although water is the type of material continuity. As to impenetrability in the molecule we can predicate nothing. The evolution of heat in chemical combinations indicates penetration of volume, with reorganization of the molecule in less space; and there is no reason, except a scholastic one, why two or more molecules, or even atoms, should not occupy the same place, as admitted by the highest authority—James Clerk-Maxwell.

Dimension is also a common notion, derived similarly from supeficial and early experience. Solids alone have figure and assignable dimension, though liquids have fixed volume, and gases variable volume, in inverse ratio to constraint; but even solids are of varying and fluctuating dimensions, according to temperature, density, etc. Solidity and liquidity are, it is well known, but mere transitory conditions of material aggregation, for all matter is capable, by sufficient accession of molecular motion, of assuming that hyperbolic or expansive condition which we call gaseous, and in this state dimension and impenetrability are meaningless terms. Concerning dimension as a necessary attribute of the unit of mass, Clerk-Maxwell says (Encyclopædia Britannica, 9th Ed., Vol. 3, p. 37): "Many persons cannot get rid of the opinion that all matter is extended in length, breadth, and depth. This is a prejudice * * * arising from our experience of bodies consisting of immense multitudes of atoms." That there is no necessary relation between mass and volume as there is, *e. g.*, between mass and weight is shown to common experience by the notably different masses of a buck-shot and a pith-ball of the same dimensions, or of a cannon-ball and a child's hydrogen balloon. A pellet of iridium equivalent in mass to the pith-ball might be microscopic, and, by extreme supposition, infinitesimal. We are not forced, however, to deny to

the unit of mass finite magnitude, as this would be an experiential fact when ascertained.

The remaining so-called properties of matter are too obviously transitory, accidental, or derivative to require attention. Color, luminosity, opacity, transparency, sapidity, sonority, odor, texture, temperature, diathermancy, plasticity, hardness, brittleness, density, compressibility, conductivity, malleability, fusibility, solubility, and many others, are too clearly but conditions of aggregation, or else mere subjective states due to the way the complicated interactions of the primary qualities affect our senses. What are the primary qualities?

Here is where the modern method of philosophy flags, by the disappearance one by one of the experimental means of approach, as we eliminate the non-essentials. But though the substance is thus elusory, we cannot yet believe it to be illusory.

Chemical and molecular physics have already gone marvellously beyond the ordinary range of sense-perception, by strictly scientific methods. Not only is the discrete character of matter established, but many data of the differentia and organization of the molecule are discovered. Here is a vast field of science in itself. From the ideal molecule, or simple couple, up through the 70 actual organized molecules of our provisional elements, then the chemical molecules of their combinations in vast numbers, discovered and undiscovered, and, lastly, the enormously complex organic molecule in infinite variety, the domain transcends in area for classification that of biologic science. The simple molecule has not yet been discovered, much less the molecular constituent, the atom, or the *indivisible.* It is evident, however, that the properties of matter which are essential, not differential, must reside in the atom. The philosophers succeeding Newton treated the atom and the elementary molecule as one, from lack of sufficient chemical knowledge. We are on a higher plane of information, but their method is not necessarily vitiated by such lack of distinction.

We cannot, as before said, attribute *à priori* to the atom dimension or figure, though we postulate it to aid conception. As the atom is an absolute unit, there is incongruity in finally assigning to it such relative attributes, which are but matters of comparison and degree. There are properties, however, which are inseparable from an absolute essence. These are the properties by which the

essence is manifested to us. We know them provisionally as forces, in the Newtonian nomenclature. Had gaseous matter neither weight nor mass, we could not know of its existence. But these attributes are so constant in matter that we estimate its quantity in terms of them and have no other exact terms. Weight is the statical measure; mass the dynamical measure. And since weight and mass correspond for all substances, under all transformations, we judge that the correspondence identifies them alike with the essence. They cannot be the mere result of organization. They must belong to the ultimate atom.

At this point it would seem proper to attend to a question of definition. Definitions are essential to clearness, on the one hand, and a source of entanglement on the other, if we fall into the scholastic error of regarding a mere word as the coextensive symbol of an idea. Words are evolved during the imperfection of ideas, and language is still a most imperfect medium of expression. Hence, logic is not a science in the sense that mathematics is. I have used the term force. This is a word of much ambiguity of meaning. We may use it as a convenient mathematical expression for a mere rate of change of momentum, or we may go farther and *define* it, as that which changes a body's state of rest or of uniform motion in a straight line; either of which uses restricts it to only a portion of phenomena, and ignores the whole science of statics, dealing with forces in equilibrium and the phenomena of balanced stress. If we give it a more general signification, as that which changes or tends to change, or conserve, the state of motion of particles, or systems of such, either in quantity or direction, we embrace statics as well as kinematics, and get a measurably philosophical definition, if we bear in mind the proviso that we do not thereby postulate force as an entity apart from substance.

And since the compound variable space and time condition which we call motion (of which rest is but a phase) is the sensible resultant of the interaction of such discrete substance by constant rearrangement where readjustment is free, or the potential resultant where confined, we may admit that the observed tension and persistence, of whatever form, is that which effects the phenomenon (though masked by infinite variety and composition), and always across the discontinuity: not as separate entities, but as modes of manifestation of the interacting and pervasive substance itself and

its only manifestations. This we call *force*—the inscrutable agent of phenomena—and this I take to be the true Newtonian conception, as evinced by his maturest conclusions, expressed in query 31 appended to his Optics. (B. 3, 2d Ed., 1717.)

So far as weight goes, it was generalized by Newton to be a reciprocal force or stress, operative without limit on the law which inheres in radial space relations—the inverse square of the distance. The term operative means effective upon mass, namely, bridging the discontinuity. Gravity is the typical attractive force—*vis centripeta*. The relation is mutual by the law of action and reaction, and amounts to a universal tension among particles, controlling all matter everywhere into orderly movements and relations. This is what we postulate from observation, on the Newtonian plan of naming simply what we see. The notion, however, of action at a distance has encountered a metaphysical difficulty in many minds, from the preconception derived from ordinary experience that all affections or stresses must proceed through an intermediary connection, deemed continuous. Even Newton made concession to this prejudice in his oft-quoted letter to Bentley. That there is really no such continuity in any mode of connection known is demonstrable, and the notion itself that the fancied continuity of some rare effluvium could in any way aid the mechanics of the problem is chimerical. Clerk-Maxwell, moreover, has shown (Nature, Vol. 7, p. 324; Encyclopædia Britannica, Vol. 3, p. 63) that action at a distance is as necessarily implied in repulsion as in attraction, so that theories of repulsion do not aid conception. Ability or inability to conceive, furthermore, is not held even by the metaphysicians to be a criterion of objective truth. Such truths exist independent of the conceiving mind. The conceiving organ was evolved by experience, and conception develops with attention. The first law of motion was wholly inconceivable to the contemporaries of Galileo, and we find such instances even now. Thus, while plain truths are inconceivable until established, some utter absurdities have been deemed conceivable, as, for instance, vacuity of two dimensions. State of mind, then, is no measure of external truth.*

* In this connection, to illustrate how entirely a matter of opinion or prejudice or culture is this notion of conceivability, I quote from a letter

The second force or manifestation of the atom, inertia,—or mass,—unlike gravity, is not unlimited in range of action. As to this property matter is discrete. Mass has both a *locus* and a limit (being apparently dependent for dimension on multiplicity), and amounts to that incomprehensible property by which conservation of motion is maintained. Under gravity, quantity of motion varies according to relations of contiguity, but under inertia motion is conserved in direction and quantity, is modified in direction and quantity by interaction of mass with gravity, and is redistributed by interaction with repulsive force upon an indefinitely near approach of particles, upon conservative principles. Its discreteness gives matter its numerical and finite character, and admits of that interplay which constitutes phenomena.* Its reality and primary

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written by Faraday to Dr. Playfair, in response to some inquiries of the latter about his atomic opinions:

* * * "I believe in matter and its atoms as freely as most people—at least, I think so. As to the little solid particles which are by some supposed to exist independent of the forces of matter, and which in different substances are imagined to have different amounts of these forces associated with or conferred upon them, * * * as I cannot form any idea of them apart from the forces, so I neither admit nor deny them. They do not afford me the least help in my endeavor to form an idea of a particle of matter. On the contrary, they greatly embarrass me; for, after taking an account of all the properties of matter, and allowing in my consideration for them, then these nuclei remain on the mind, and I cannot tell what to do with them. The notion of a solid nucleus without properties is a natural figure or stepping-stone to the mind at its first entrance on the consideration of natural phenomena; but when it has become instructed, the like notion of a solid nucleus apart from the repulsion, which gives our only notion of solidity, or the gravity, which gives our notion of weight, is to me too difficult for comprehension; and so the notion becomes to me hypothetical, and, what is more, a very clumsy hypothesis." (Playfair's works, Vol. 4, p. 84.)

Here we see a difficulty opposite to that usually encountered, for, while many people profess an infirmity of conception of the forces apart from the imaginary vehicle, Faraday finds the vehicle of no use as a carrier of the properties, but a positive impediment.

* This property has a multiplicity of names in the Newtonian nomenclature, according to the varying aspect of its function. Thus, in the aspect of persistence of mass in state of rest or of motion uniform in direction

character, when once apprehended, have proved more acceptable to the imagination than has the conception of central force, and under appulsion hypotheses (with the aid of that other readily accepted property, repulsion, and certain highly artificial hypothetical media), it has been made to do duty in providing so-called explanations of gravity, under its form of *vis viva*.

It has always seemed to me that the mode of approach adopted by Boscovich was the most philosophical and rigorous of any. He viewed matter for the purposes of mathematical treatment and for investigation of its essentials, as divested of accidental and fugitive properties; and as the analytical calculus had not then become so developed as to wholly fascinate the attention of geometers with abstract and ideal relations, he proceeded from prime physical data. He thus identified matter by those apparently general and characteristic properties recognized by Newton as the basis of mechanical philosophy in conjunction with the laws of motion. These properties are, as before said, gravity, inertia, and repulsion; or, as characterized by function, attraction, conservation, distribution. In this view matter consists of certain *loci* of central forces, mutually attractive by the first property according to a variable law in the duplicate inverse ratio of distance without limit, but restricted in manifestation as to the second property to the infinitesimal *locus*, thereby excluding unitary dimension. Contemplating matter under this aspect alone, a dilemma arose. For gravity waxing by the law of inverse squares of the distance up to the focus or origin involves the consideration of infinite force and apparently of infinite velocity in the limit, in the supposable case of rectilinear ap-

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and quantity, *i. e.*, of resistance to change of state except in conformity with motion impressed, the property is called *vis insita*, which may be *vis insita activa* (momentum), or *vis insita passiva* (*vis inertiæ* of mass.) In its aspect of acquirement of a new state of motion by interaction with other forces or masses, Newton called the new state thus superposed *vis impressa*; which, when the operation of acquirement has ceased, becomes again *vis insita*. In its aspect of persistence of mass towards uniform direction of motion under the constant deflective stress of vector central force, it is called *vis centrifuga*. And in its active form, conditioned by motion acquired, its capacity for furnishing motion from its store, either for impressing motion upon other mass, with consequent loss, or for supplying the potential fund under the drain of adverse central force, is called *vis viva* (energy.)

proach, at which point the equations become unexplainable. While Euler and La Place differ in their interpretations of the result, Boscovich sought to solve the apparent absurdity and inconceivability by the invention of his ingenious and complex system of alternate spheres of attraction and repulsion, or change of sign, on a very near approach, with infinite repulsion at the focus, which so loaded down and vitiated his hypothesis as to cause its rejection. This result was similar to that of Le Sage's speculations and those of the Ptolemaic astronomers, each thus working out the falsity of his respective scheme by superadded complications to readjust the theory to the progress of criticism or of observed fact.

By attributing finite magnitude to the atomic mass, however, Boscovich's difficulty disappears, as I had the honor of pointing out before this Society some ten years ago. This may be deemed a violent hypothesis in regard to a positive discrete simple absolute, as the atom is presumed to be, but parallel difficulties inhere in any other finite supposition, as, *e. g.*, a sphere of repulsion. Under my provisional assumption, the way out follows from an elementary proposition of Newton's, and it does not demand the gratuitous change of law or of continuity involved in the resort of Boscovich. The movement of a gravitating particle under stress of a center of gravitative force would be in all respects as the great 18th century mathematicians have demonstrated, until the margin of the particle reached the attracting center, where, if we suppose the attractive virtue to prevade the particle equally throughout a certain finite volume of mass, however minute, as gravity does the mass of a sphere, the maximum of attractive force would be attained; for, as Newton has shown, homogeneous spheres are controlled under gravity by a law of force varying directly as the mass and inversely as the squares of the distance between their center of mass and the attracting center, at all points *beyond* the surface, and directly as the distance between the said centers *within* the surface; so that, after passing the surface, the attractive center must proceed onwards to the gravitating center of mass (relatively), not by a force increasing to infinity, but by a force decreasing to zero, after passing the maximum, since it is balanced at the center by opposing stresses.*

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* Let M be an exaggerated particle of mass and C a fixed center of gravitation external thereto. Newton proved that for all positions outside of a

A similar law of attraction prevails between two gravitative particles when both are similarly endowed with finite spherical volume and mass, excluding the idea of impenetrability (which is not a necessary attribute of mass), the Newtonian law being the product of the masses divided by the product of the distances $\left(\frac{Mm}{dd}\right)$* for outside positions.

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gravitating homogeneous spherical mass the stress is precisely as though the whole mass thereof were concentrated at the center of said sphere, and varies directly as the mass and inversely as the square of the distance between the said center and the fixed center of gravitation; *i. e.*, $G \propto \frac{M}{d^2}$. The maximum of gravitating force will here be at the surface, where d is minimum. He also proved that at all points within a homogeneous gravitating spherical concentric shell a gravitating particle is uniformly affected by balanced attractions. Hence, the stress for any smaller concentric sphere is $g \propto \frac{m}{r^2}$, m being the smaller spherical mass and r the reduced radius. But since homogeneous and similar masses are as the volumes, and similar volumes are as the cubes of the homologous dimensions,

$$m \propto r^3. \therefore g \propto \frac{r^3}{r^2} \propto r.$$

The maximum of gravitating force is here also at the surface, where r is maximum.

*I write the formula this way because it is possible that we have been in error all along in regarding the denominator as a radial space relation, as implied when we write it $\frac{Mm}{d^2}$. In discussing the deflection of the particle under gravity, Newton, for mathematical simplicity, treated it as governed by a fixed attracting central force, and in testing various relations found that the radial space relation gave the true path of the planetary bodies under the immense preponderating influence of the sun's mass. The fixed center of attraction is, however, a mathematical, not a physical, condition, and can only be realized by making $M = \infty$, when we get a form of expression which does not give a law of force. I think it possible that the relation is a mere reciprocal distance relation, since the stress is mutual for the masses and each is equally distant from the other. The inverse form of the relation, moreover, may arise from our subjective way of viewing distance, as measured outwardly from ourselves, since we have to go from here to yonder. It is possible to look upon the relation as really one of contiguity or nearness, and by placing $\frac{1}{d} = c$ we get the cosmical law of gravitation as *Mcmc*. This, however, would not be a useful formula, since we are not accustomed to expressions which attain maximum value with minimum magnitude.

For positions of encroachment the law is more complicated, and forms an interesting field for mathematical discussion. Where three or more atoms are superimposed the problem becomes too complex for discussion. It is noted, however, that such compound atom, if quiescent from extreme abstraction of heat, would be in a condition of elastic equilibrium, ready to respond like a bell to the slightest disturbances. In all these cases of interpenetration the law of stress would be finite and diminishing, and if the line of encounter should chance to be a right line through their centers (a condition infinitely rare in actual occurrence), they would continue on or repeat according to energy of approach; while upon any other lines of approach orbital relations would supervene, in modified curves of the second order, either hyperbolic, parabolic, or elliptic, according to velocity, and with or without partial penetration, according to nearness of approach.

Boscovich, however, did not adopt this solution, although within his reach. The problem of the action of a gravitative particle as controlled by an attractive center has several aspects of statement, which may be confined to four, for practical investigation. In the first, where the particle is assumed to be without mass, no discussion is possible, for the two suppositious points instantly assume the same locality, and end the relation. In the second, where the particle is endowed with inertia but not magnitude (and the attractive *locus* fixed by postulate), the element of motion enters, but infinite terms appear in the equations in the limit, forbidding interpretation. Thirdly, when we attribute finite magnitude to the gravitative particle for gravitative pervasion, as in actual spherical masses, no infinite terms appear, and we get an intelligible mathematical discussion, with planetary results for exterior positions, and pendulum results for interior positions, as I have heretofore demonstrated; and lastly, when both the gravitating *loci* are invested with similar attributes of volume and of mass (excluding extraneous notions of ordinary collision and repulsion from the problem), the results are similar to those of the third hypothesis. I do not introduce any of the mathematical discussions here, as the dynamics of the particle have been fully treated by mathematicians, though I am not aware that any of them have pursued it to physical conclusions.

It is not likely, however, that there is any matter so simple as this modified Boscovichian atom; that is, which can be identified.

All the matter we know of is already compounded and highly organized. The ideal simple molecule would consist of a single pair of such atoms, bound to each other in orbital relations of more or less eccentricity, including the extreme rectilineal form of simple pendulum-like oscillation through one another's centers; and it is a most significant fact that spectroscopic observation of all incandescent matter shows atomic matter to be in this state of transverse or orbital oscillation with inconceivable but synchronous rapidity without regard to range, according to the pendulum law of stress varying directly as the range of oscillation, discovered by Galileo. Any theory of the simple molecule must take cognizance of this observed fact. Another cognate fact is that the law of elastic cohesion manifest in all elastic tensile action—"*ut tensio sic vis*"—is a parallel law of stress, as illustrated in the spring balance weighing scale, the spring dynamometer, the isochronous spring governor, etc., and is a function of molecular and ultimately of atomic force and distance.

If the atom is really thus characterized, the repulsion or resistant property experienced in matter becomes worthy of investigation, since it drops out as the primitive affection or disaffection postulated by Boscovich. I have shown that it is not necessary to oscillatory motion. We must admit that the notion of rebound or recoil, in the ordinary sense, between simple atoms possesses difficulties. No less does the idea of plasticity or destruction of momenta. Consider what is involved in the hypothesis of two absolutely hard, rigid, unparticled, homogeneous spherical bodies of any magnitude at all, if possessed of mass, meeting on a rectilineal central line of motion. We know what would happen in case of ordinary spherical elastic masses or aggregations of molecules. Such merely undergo, first, apparent contact, then compression, deformation, strain, accumulation of stress, retardation of velocity, momentary arrest, acceleration on new lines of departure, relief of strain, recovery of form, redistribution of momenta, and final resumption of uniform velocities, with relative motion inverted and aggregate energy of motion unimpaired, unless permanent distortion and heat have absorbed a portion. All this complex action is involved in the term elasticity. None of this could take place with simple undifferentiated particles, unless we invent for them a mystic atmosphere or cushion of repulsive capacity surrounding the *locus*, as Boscovich was forced to do

by logical conclusions. Without this, contact would be absolute and instantaneous at first impact. As hardness involves impenetrability, absolute destruction of motion on the instant must ensue; that is, motion and no motion at consecutive instants of time; a discontinuity unknown to experience, and known to be inconsistent with the nature of motion and of time. This argument from breach of continuity is due to Leibnitz. Conversion into heat motion is excluded, heat being a mode of motion of the entire atom. Moreover, the destroyed motion has to be recreated instantaneously in new directions, for destruction of energy cannot be postulated. This geometrically angular motion is also unknown to experience, for all deflected bodies pass by continuity from motion in one direction into a new direction, and, so far as we can see, must do so. These discontinuities in translatory relations are therefore put aside, not because they are inconceivable, but as illogical and non-experiential. Simple repulsion by contact without occult intervention is a false suggestion, and we find that we get the pseudo-conception from our false observation of what occurs in the collision of sensible masses, somewhat as we make a false observation and generalization about material continuity, or about tension, from a superficial perception of matter; thus creating concepts from supposed experience which can have no true objective counterparts. I shall recur later to a possible derivative basis for repulsion.

It is remarkable that to Newton we owe the final establishment of the majority of those fundamental and universal truths which by simplicity and generality seem to touch the absolute; that is, more than to any and all other philosophers combined. Thus, of the six ultimate generalizations, four were formulated and placed on an impregnable basis by Newton: the three laws of motion and the law of gravitation. All of these were inconceivable when first promulgated, were hotly controverted on the metaphysical plan, were finally established experientially, and are now generally accepted as axiomatic by the modern mind, except for sporadic reversions which appear now and then to deny their actuality and reassert their inconceivability. The remaining two universal inductions are the collective group of axioms formulating the relations of extension—the only enduring remnant of the Greek philosophy—and the law of the conservation and unity of energy, unperceived in Newton's time in its generality, though taught as a dogma by the

Cartesians. These also are still held to be inconceivable by certain disciples of metaphysical methods and axiomatic by others. Such mental attitudes should lead us to believe that simplicity has been arrived at in all these cases and the boundaries of explainable knowledge reached, where inconceivability necessarily begins.

It has been said that paradox is born either of confusion of thought, or of knowledge, or confusion of statement arising out of the imperfection or subtlety of the verbal vehicle of thought. Thus, as Clerk-Maxwell points out, the celebrated arguments of Zeno of Elea, establishing the inconceivability of motion, represented in the paradox of Achilles and the tortoise, were unanswerable and unanswered until Aristotle showed, some half century later, that duration is continuous and incommensurable by numerical methods in the same sense that extension is. The old logical dilemma of the irresistible force encountering the immovable body was insoluble to the Greek mind, both from lack of physical knowledge and lack of verbal clearness of statement. The acute sophist knew not the nature of force, the constitution of bodies, the conservation, transformation, and dissipation of energy, and consequently knew not the refuge and escape from the dilemma contained in the perception of the conversion of molar energy into heat energy, expansion, and dissipation. The resources of verbal subtlety and of inner consciousness failed, as they always do. Something of the same difficulty remains in modern problems, where observation and strict verification are, from the nature of the problem, inapplicable, or where the confusion arises from the still-existing imperfection of language, or, again, where generalizations, both clearly made out and clearly formulated, have not passed into the instinctive popular apprehension. The modern dilemma of the inconceivability of infinite or finite space is, I take it, due to the metaphysical form of the statement. For when we reflect that the ideas of immensity and of infinitesimal resolvability are but abstract generalizations of the merely relative continuities, extension, distance, and dimension, which are in their turn but abstractions of the sense-perceptions, form, translation, and volume, the statement becomes intelligible and entirely conceivable, and I think, though with deference, saves geometry; that is, the universality of that system of inductive postulates regarding the relations of extension and inferences therefrom, known as geometry to the Greek philosophy,

but now named Euclidean by certain analysts whose so-called geometry is symbolic. Geometry is therefore able to deal with all aspects of extension, without regard to limit, in spite of some infirmity in the Greek method, for scale cannot affect the generality of extension relations, and abstract unconditioned space is not an entity but a mere negation, concerning which relative propositions are unintelligible. A false philosophy regarding space is at the root of all modern heresies concerning geometry and mensuration, founded in misapprehension of the Euclidean inductions or generalizations.*

The first law of motion is but the formulated recognition of inertia, which is only manifest in conjunction with motion, actively or passively. It was known to Galileo, and laid down by Descartes as a law in his Principia. It is a cosmical truth, bound up with the absolute nature of mass and the true relations of extension, which correlates the whole fabric of dynamical knowledge with rectilinear geometry, curvilinear motion being demonstrably not a simple state of conservation under inertia, but a resultant of multiple forces. The simple action of mass under the first law of motion, if undisturbed, furnishes the absolute unreturning rectilineal path which overthrows all speculation about possible ideal spaces. I here recall a book written by a learned American of Philadelphia—learned, that is, according to the mediaeval standard of the colleges—and published only during the past year, en-

* There are two opposite though similar forms of error in the assumptions regarding space. The first is that space is a specific or perhaps generic entity or objectivity *per se*, possessed of conditions and attributes, like substance, such as dimension (in several), differentia in locality, figure, as curvature, etc. (hence necessarily finite), and only uncognizable by us simply for lack of perceptive faculties to correspond. This is the fundamental error, as it seems to me, of Riemann and Lobatschewsky. The second is that of the older Cartesians, who viewed space as but the mere attribute or synonym of substance, and inconceivable apart from it, so that bodies separated by void space would be absolutely in contact without regard to distance. Both of these speculations are purely metaphysical, and non-experiential, the latter resulting from the old scholastic method of syllogistic deduction from primary postulates of verbal definition, and the former from similar inferences from the forms of the analytical logic of symbols, the use of which is still in the scholastic stage. Like Zeno's paradox, these merely intellectual difficulties should be removable by intellectual processes.

titled "An Examination of the Philosophy of the Unknowable, as expounded by Herbert Spencer," wherein he naively lays down the first law of motion as unintelligible except by appulsion. Motion, he says, in the absence of propulsion is inconceivable. I have no space here to reproduce the explanation evolved out of consciousness by this reasoner to account for the action of a ball struck by a bat after leaving the bat. It resembles in ingenuity and gratuity some of the inventions devised to explain gravity. The notable thing about it is that here, at this date, is a mind of good caliber, informed in the higher schools of learning, which is still of the mental period of Aristotle; a mind which has evidently never apprehended inertia, nor heard of the great contributions to knowledge made by Galileo and Newton, by which philosophy was entirely revolutionized.

The second law of motion, regarding the independence and coexistence of motions, on which we occasionally see comments in the metaphysical vein controverting its possibility, has long been established experientially. Its early experimental proof is attributed to Galileo. Yet I recall a pamphlet written and published only during the last year by a learned German at Leipzig, the theme of which was that "the sun changes its position in space, therefore it cannot be regarded as being in a condition of rest." This, he concludes, overthrows the entire fabric of Copernicus, because the planetary orbits in such case cannot be closed.

The third law of motion is but formulated reciprocal stress, in its modes of compulsion and repulsion, through which mass acts on mass to redistribute motion by what appears to be necessary law. The stress is necessarily reciprocal, since there is no *point d'appui*, or fixed fulcrum, in the universe.

We have thus been brought to the boundary of the absolute, where all is inconceivable until found out, and where the simple data are unexplainable. All examination seems to continue to point to mass and weight as the ineffable simple insignia of substance standing on this limit. We must accept something as elementary fact; what shall we find more elementary? Repulsion is still debatable; for, if we make an issue between repulsion and compulsion as contradictory primary attributes of the same essence, or untenable in conjunction for artificiality, by far the greater difficulties attach to the former, some of which I have already alluded

to. The profound mind of Boscovich was forced to accept repulsion as a primal quality, but in deference to the physical hypotheses of his time, he overloaded it with complication. This has been weighed in the balance of philosophical judgment and found wanting. I have intimated that there are possible grounds for surmising that it may not be a simple property of the atom, but a mere mode of distribution of energy dependent on composition of motion of atomic mass after change of sign, *i. e.*, a mode of *vis impressa* after exhaustion of the space relation; for, mathematically, the hyperbolic lines of approach and recession of two atoms under the high proper motion characteristic of the atom, and on lines not directly central, would be similar, at sensible distances, in their asymptotes (which would be the practical paths), whether the deflection were due to attractive or repulsive stress, though acceleration and retardation at the passage of the infinitesimal focus would be inverted.*

* It is well known that for any finite system of two particles controlled by gravity the lines of movement are closed curves of the second order, of more or less eccentricity, about the common center of gravity, which, for equal masses, would be midway. For an infinite system under the same conditions the orbits are parabolic, but for a system to which the particles enter by extraneous motion the lines of movement are hyperbolic, thus:

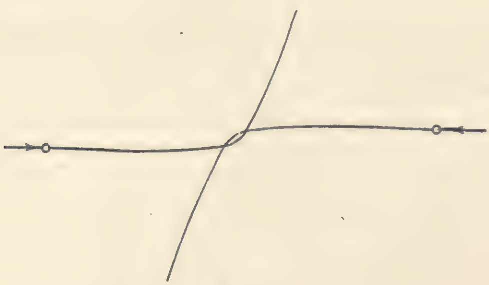

FIG. 1.

Now, under repulsion, the lines of motion are seen to be similar, A B, D E, Fig. 2, being asymptotes of the hyperbolas representing the two paths at sensible distances:

It therefore seems to me immaterial to result which of the two modes of passing the infinitesimal focus is the true one. In either case the distance at passage is infinitesimal, and the force may be as near infinity as the facts require it to be assigned. The normal or rectilineal encounter is here excluded from supposition. In that case, under repulsive stress, as postulated by Boscovich, the recoil would be rectilineal and opposite, without breach of continuity. Under attractive stress, with finite volume of the atomic mass, penetration would ensue as before shown; but without dimension or repulsion we have an insoluble condition, although the occurrence would be infinitely rare. Only one pair of elements is here considered. In all real encounters, whether of masses or molecules, the effect is a vast resultant, but should not be different in kind from that of the elements; that is, hyperbolic or expansive between alien systems under motion. As the number of elements ordinarily engaged could not be represented by any numerical places of arabic notation for which we have names, we see the hopelessness of stating the problem mathematically. I therefore do not presume to

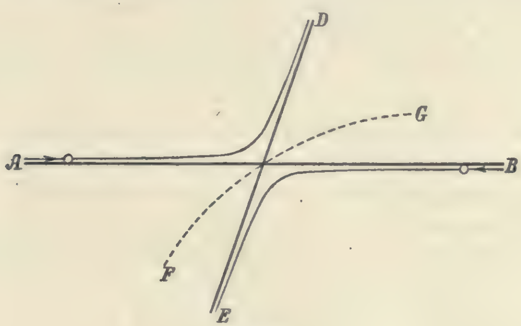

FIG. 2.

This encounter represents only one element of the molecule, of which myriads are engaged at every recoil of molecules, not to speak of solids. It is thus seen that the mesh constituting the molecule is ordinarily impenetrable to other meshes. If the curve F G be allowed to represent the outline of the molecule, the limb of the solid to which it belongs, say a buckshot, will be represented by the Sierra Nevada, or the Andes, and its diameter would be measurably represented by that of the earth, as approximately shown by Sir Wm. Thompson in the case of a drop of water.

offer this as an explanation of repulsion, and I confess that to me repulsion is in its mechanism incomprehensible. We know the result experimentally, and that is resistance to penetration, and reaction at insensible distances on an undefined boundary which begins prior to contact and increases in a high exponential ratio as approximation progresses. The contact boundary of any solid—even the smoothest and hardest—resembles the astronomical limb of Jupiter in geometrical indefiniteness. The contact transmitter in the telephone, the whole range of whose phenomena occurs under pressure and so-called contact of varying degrees, illustrates how relative a thing is contact. Under high velocities the distinction between solids, liquids, and even æriform bodies entirely disappears in respect to repulsive reaction, though this is the most sensible distinction between them under low velocities.

We may, therefore, adopt the conclusion that if any of the apparently simple properties of the atom are to be thrown out as derivative and secondary, presumption points to repulsion as the complex one. We could possibly account for phenomena in a universe bound together by purely tensile stress, but most of the sensible phenomena of solids—cohesion, affinity, tenacity, etc., including nearly all of statics—remain hopelessly unattackable problems under a hypothesis of pure repulsion, like that of Le Sage, or Preston. It is to be noted that the kinetists who freely postulate repulsion and appulsion, without analysis, as a primordial fact, but reluct against compulsion or tension, are forced to the invention of the most complicated and gratuitous mechanism and media to explain the phenomenon of gravity, and then without attainment of result. Le Sage's atom is too complicated, even without his suppositious or extra-mundane operative machinery; and the vortex atom is but a mere analytical expression for an unproducible condition in a figmentary mathematical plenum.

The thesis that conservation is the characteristic by which we identify objective existence will not bear the test of examination It is only in the most recent times that such a quality has been known or imagined, and its establishment, both as to matter and energy, is justly viewed as the triumph of modern philosophy. The evocation of matter from nothing and its relegation to nothing, even by the finite will of a wizard, was ever a common and universal notion, which did not at all impair the belief in its present reality

and substantiality. We have only to go to Apuleius for this, and it is doubtful if even now the notion of the indestructibility of matter is anything but a scientific conviction, for do we not see numbers of our contemporary fellow-citizens meeting together frequently in our midst to witness feats of materialization out of nonentity by powers akin to those of the sorcerer, without an idea of incongruity? Nor has the essentially modern doctrine of the conservation of energy anything to do with the belief in its reality. Few people apprehend it even now. No philosopher understood it a hundred years ago. Its verity rests on a sufficiently general inductive basis, from the refined and exhaustive experiments of Joule, and the theoretical conclusions of Mayer and Clausius, and it is accepted in the same sense that the law of gravitation is accepted. But the duality of matter and energy to the exclusion of force is a verbal shift, the assumption of which removes no difficulty. Matter, the object, remains unexplained; and energy, the phenomenon, becomes segregated and unintelligible. Energy, in fact, is but mass in phenomenal manifestation, being a product of triple factors, two of which—translation and speed—are not things, but variable and evanescent conditions, and, taken together, constitute motion. Mass is the absolute or persistent factor, but the evanescent character of the variable component—motion—would render the entire phenomenon—energy—apparitional, were it not for the distance relation involved in motion, which, under the same inscrutable agency which modifies and saps the motion renders it potential upon change of sign. This agency, the dynamical source of the manifestation, being central to mass and likewise persistent and constant, renders the positive and negative potentialities of movement constantly equal, and the actual and potential energies consequently complementary, from which energy gets its character of conservation.

Energy cannot therefore be that other reality of existence (besides matter), since force is clearly the one reality at the bottom of the manifestation of both, to whose persistence and resistance to change, except through transformation, the conservation of both is due. This one reality is, in its triple aspect of causation, (1) attraction—the source and modifier of motion; (2) inertia—the conserver of motion; and (3) repulsion—the distributer of motion; or, more correctly, in its aspect of quality: (1) *vis centripeta*—the power of mutual control across distance; (2) *vis insita*—the power

of persistence in state of motion impressed; and (3) the distributive power of imparting and acquiring motion by transfer, at minimum distance, which may be called *vis partitiva*, the result of which is Newton's *vis impressa*. Matter thus comes into the world of phenomena by the simple presence of other matter, permitting the exhibition of these comparisons and interactions, involving the conditions of contiguity, distance, position, translation, direction, succession or sequence, and time-rate for the continuous increments, decrements, successions, and uniformities, all bound up in the compound variable continuity—motion. With motion and distance comes the dependent phenomenon—energy—active and potential, which should be a constant, the numerical units of mass being constant throughout immensity, provided the sum of the motions, potential and actual, be constant. This the dynamical theory deduces from the fact of central force (for without force potential motion is ridiculous), and the thesis of the conservation of energy is a dynamical truth or nothing. It is therefore all the more extraordinary that certain kinetists, who reluct against central force, should have selected, out of all the manifestations of the universe, the variable and conditional product—energy—to be the one reality or objectivity, aside from the undefined hypostasis—matter—as a primordial simple fact at the basis of phenomena. It has been mathematically demonstrated by Mr. Walter R. Browne (London Edinburgh and Dublin Philosophical Magazine, January, 1883, p. 35) that the conservation of energy is true if the material system is a system of central forces, and is not true if the system is anything but a system of central forces. In fact, the ordinary theoretical proof of the principle of the conservation of energy assumes the forces acting to be central forces, *i. e.*, reciprocal stresses between units of mass, as recognized by Clausius in his Mechanical Theory of Heat. Moreover, the entire body of kinetists, who have aimed to supersede gravity or central force, have freely assumed an extramundane supply of motion and energy without regard to conservation, and it is notable that every hypothesis for this purpose yet broached involves the constant expenditure of work without recovery, and postulates the accession of energy in infinite influx from some occult source, of which only a small portion relatively is available or manifest in observable phenomena, thus violating all three of the canons of philosophical ascription—true cause, sufficient

cause, and least cause. Such is the power of conception of the unknown in endeavor to explain the inconceivable known.

If the dynamic hypothesis of perpetual transformation of energy could be established as a universal induction, with as much generality, *e. g.*, as the statement of the law of gravitation, it would establish and confirm that law, by Mr. Browne's demonstration, as something more than a law, to wit, the necessary constitution of matter as a system of central forces and nothing more, substantially as conceived by Newton and elaborated by Boscovich. At present it is but a dynamic induction, but the theory of gravity is no more. Our appliances are material, and we can deal with molar forces, but only indirectly and inferentially with those which are atomic. Conservation is indubitably true of energy in the mechanical and molar sense, under the laws of dynamics and the persistence of force. It is, also, experimentally true, so far as we can trace it, of those less understood forms of energy which are molecular or atomic, the establishment of which was the great glory of Benjamin Thompson, Clausius, and Joule as to heat, and of a multitude of observers as to electrical energy. We infer it as a general truth of these energies (formerly known as imponderables, since they are not manifestations of matter in the concrete), from the fact of their convertibility with other modes of energy which are undoubtedly dynamical, and also from the intimate connection of electrical energy with one of the specific exhibitions of central atomic force—magnetism. Such clews create a warrantable presumption that the phenomena in question will all ultimately be classified among the modes of atomic mass and motion, inductively as well as hypothetically. Possibly in the investigation of these evanescent modes of energy the missing simple particle may come to light. Provisionally, we are entitled to rank them among the mechanical modes of energy, as products of the same material forces, assuming, until the contrary is proved, that some form of matter is concerned in manifestations so correlated by conservation with undoubted material activities.

In including the imponderables within the general dynamical law of conservation, we have to take account of the phenomenon of dissipation, first pointed out by Sir William Thompson. It is true that heat (as well as electrical energy) is strictly correlated with and interconvertible with energy of mass motion, as before

stated, but in its final form energy seems to take leave of matter altogether, so far as our perceptions can follow it, and disappear as a material phenomenon (though liable to reappear wherever matter is encountered whose particles are deficient in a like species of atomic motion with that which disappeared; which fact indicates that atomic mass is still a factor, with its inherent property of persistence and transference). The earth and all upon it is radiating heat energy away into space at the constant rate of 500° F. of absolute temperature, more or less; the sun and the visible stars at the rate of many millions of degrees. Much energy also passes off in the luminous form. Of electrical and actinic energies we know less, and of some we doubtless know nothing. This amounts to a constant drain of the dynamical supply of energy. These final forms, the radiant energies, have a remarkable specific high cosmical velocity of their own, which is a function of something not material, or at least not molar. It is supposable that, in addition to the dynamical source of motion from central forces, and the contraction of systems in dimension which supplies dissipation, there may be an inherent and primordial store of atomic motion. The high proper motion of some of the stars, beyond what can be accounted for on dynamical principles, and the inexhaustible and enormous supply of radiant energy from the visible stars, have afforded grounds for such a surmise, but these speculations do not belong to the domain of mechanics.

And here we must bear in mind that the dynamical theory, in placing these assumed agencies and modes of interaction in causal relation to phenomenal motion, by no means predicates or can predicate anything concerning absolute motion or its cause. The lack of this distinction may have proved a stumbling block to some in comprehending the idea of force. Were it not for the observed dissipation of energy no system could become contracted in dimensions a particle by the interactions of material forces, nor is there now any known way by which the material system can be expanded in dimensions except by the accession of motion from extra-mundane sources, which there is no scientific mode of ascertaining. The sum of motions under the action of forces remains the same, and any change would imply creation or annihilation, which is not ascribable to a material agency. Primordial dimension remains as inscrutable a fact as ever, and primordial motion an unsolved problem.

In conclusion, I know nothing of force except as a manifestation of matter, and nothing of matter except through its manifestations. It is substance that interacts with substance, so far as we know, always reciprocally, and force is but the convenient translation of the terminology invented by Newton to designate these several species or modes of action, in the word *vis*, with its appropriate adjective. He was arraigned by the Cartesians (and virtually is by their modern representatives) as the reintroducer of occult qualities into philosophy, but his statement was "*hypotheses non fingo*," and to a similar charge brought against him by Leibnitz he pertinently replied that it was a misuse of words to call those things occult qualities whose *causes* are occult though the qualities themselves be manifest.

I have adopted gravity as the type of central inherent force—*vis centripeta*—but I would not thereby be understood as excluding from the category of material forces any and all other modes of tensile or constraining force which may be hereafter made out as specific, by the elucidation of such phenomena as affinity, cohesion, tenacity, elasticity, ductility, viscosity, capillarity, polarity, magnetism, etc., now so little understood, any more than I would exclude any form or mode of energy which may be observed, from the category of material phenomena. The Newtonian doctrine of force would not be impaired by such discovery, and its strength lies in the fact that it as readily includes static phenomena—that despair of the kinetist, who has no imaginable hypothesis by which to range them under a form of motion—as it does kinematical phenomena. Statical force (Newton's *vis mortua*) cannot be ignored in a theory of force. The straw that breaks the camel's back—the very lightning that crashes through the sky—are familiar examples of its power made manifest. Its reality may be exemplified by suspending two heavy balls of equal weight at equal heights—one by an elastic cord, and the other by a tense string. The difference of effort required to displace the two vertically upwards, which can be measured, makes sensible the difference between the two forms of balanced statical forces. In the one case the antagonizing force is suddenly withdrawn, and in the other gradually. Wherever strain exists—and it is everywhere—there force is as certainly present as when it becomes manifested in a stress relieved by motion and measurable in terms of energy.

Let us, then, give up the standard of *a priori* conceivability, in view of its many historical failures, and adopt as possible that which is provisionally ascertained. The "ego" and the "cogito"—Cartesian starting points—have proved barren and irrelevant in Philosophy. True Philosophy is concerned with objectivity. The data of consciousness, mainly acquired in infancy or in the womb, are blind guides. Many an ego, whose brain was his cosmos, has run through his brief subjectivity, but the order of nature endures. The same facts are continually observed, verified, recorded, and rectified, but the observers change. Their intelligent observations add to the sum of knowledge. This is all the proof we need of objectivity, and all we will get. The insoluble difficulties of Philosophy have disappeared one by one since the happy thought of eliminating them by observation entered. The immortals are those who have successfully applied this method. It is only where observation fails that the insolubility lingers. Beyond the sphere of the knowable it will continue, in spite of introspection. How masterful is fact in the presence of the most intricate mental subtleties. The ball leaves the bat, in spite of the inconceivability. Galileo's plummet dropped from the moving mast strikes the deck and not the water, in spite of the inconceivability. The Earth returns in its orbit, to the second, in spite of the sun's rapid fall through space, and of the inconceivability. Two opposed horses can pull no more than one, in spite of the inconceivability. The guinea and the feather dropped in the exhausted receiver strike the plate together, in spite of the inconceivability. The isochronous pendulum swings through the widest arc in the same time as through the smallest, in spite of the inconceivability. The minute hand overtakes the hour hand, in spite of the inconceivability. The magnet draws the iron with undiminished force through all possible interpositions, in spite of the inconceivability. Could an exception be found, the perpetual-motion "crank" would work a greater inconceivability, by the instant contrivance of a power-generating machine.

We need not aspire, therefore, to remove any of the inconceivabilities of the external world. We must accept them as natural to the finite comprehension, as necessary to faculties which act by comparison, and above all as evidences of objectivity. On the other hand we should avoid that opposite error of the introspective

school, of deeming that probable, or in any way connected with fact, which merely seems conceivable. I have shown that while the simplest truths have generally proved inconceivable until found out and established by genius, the greatest absurdities have had ready currency without a doubt of their conceivability. This all mythology shows. Such rubbish as "a thing cannot act where it is not," and "a body cannot move where it is not," or "a cause cannot precede its effect"—mere metaphysical assertions or subtleties in face of everyday fact—were stumbling blocks for ages. Such assumptions formed the basis of deduction in lieu of observation, and blocked the possibility of advance. And even yet, rigid deduction from the most hare-brained premiss, if the chain of deduction is sufficiently intricate, seems to possess fascinations over a verifiable induction, with many minds.

And now, if any ask, "*cui bono*" to the scientist, these philosophical inquiries and intricacies when he has the vast field of unexplored data still before him to occupy him, I answer, the queries of Philosophy are not only the main-spring and final cause of science (her first fruitful daughter and handmaid), but they, consciously or unconsciously, dominate the methods and results of science herself. Each investigator, even though in the domain of the most abstract of the sciences, postulates more philosophy than he is aware of; and with so much the more danger to final accomplishment if he assumes his philosophical basis without examination. It is the errors of giant minds that are dangerous, by their ponderosity. The infallibility of the master, Aristotle, seemed to make investigation useless, until the rise of parallel giants, like Galileo and Copernicus, stimulated a new conflict of opinion. And Descartes, though harmless from all his productions within the metaphysical domain, is dangerous by his very eminence and originality in science, which gives vogue and currency to his monumental errors. Although acquainted with the true law of motion, his scheme of matter evolved from consciousness would forbid all exhibition thereof. A grand geometer, he erected a scaffold for scaling immensity, and with unparalleled penetration perceived how a purely ideal logic, if general, would represent truth in a wholly dissimilar realm of deduction, if equally general. Strange to say, this grand and useful discovery has become the engine, in nihilistic hands, for overthrowing all the positive knowledge we

possess—the achievements of two thousand years of human effort. Not only geometry—all that has survived to us of philosophical value from the antique world—but the basis of positive dynamics, as handed down from Galileo and Newton and Boscovich and Dalton, are apparently undermined, for all that gives them intellectual value—their certainty—unless an effort be made in the neglected field of philosophy. With strange inconsistency these advocates *par excellence* of the experiential origin of knowledge are found in the same breath promulgating as possible truth matters not only non-experiential, but not representable in ideas derived from or verifiable by experience, and avowedly originating not from inductive generalizations—the only source of knowledge—but in purely deductive processes in the old scholastic way, from logical premises of bald assumption. In a similar way, in the hands of the Greek sophist, language, a good servant, became a vicious master, and made a chaos of all ethical achievement. A remnant of knowledge, fortunately expressed, not in verbal, but diagrammatic logic—geometry—was left, but only to fall now by the hands of similar iconoclasts, armed with more potent destructiveness, in its full flower and fruit of twenty centuries of unmolested growth.

It is time, therefore, to get back to Baconian ground, and while using for its legitimate purposes the magnificent modern machinery of analytical investigation in the field of abstract continuity—extension, motion, duration—not attempt to conjure with it as a source of objective revelation, which no mere machinery can be. A scaffold of n dimensions is as useless to the geometer as to the architect. To assume matter as continuous, simply because of the possession of a potent engine for the investigation of continuities, is to repeat the practice of certain quack specialists, who are prone to diagnose nearly every form of disease as a variety of their own peculiar specialty. And to interview the symbols of a mathematical logic for the prime definition of a fundamental objectivity, like force, is to revert to a barren source of knowledge, by an obsolete process in philosophy, and bar all progress in anything but abstract technique.

The paper was discussed by Mr. W. B. TAYLOR and Mr. KUMMELL.

Mr. T. Robinson made a communication on

THE STRATA EXPOSED IN THE EAST SHAFT OF THE WATER-WORKS EXTENSION.

[Abstract.]

The shaft (23′ square in the clear) was begun in the bottom of an old sand-pit at a level of 131.5′ above tide. This sand-pit was excavated in the side of a hill; and recent cuttings have exposed the strata from the hill-top to the level of the top of the shaft. Thus we have a vertical section of 188.5′, extending from 171.5′ above tide (or 40′ above the top of the shaft) to 17′ below tide.

1. About 6″ of surface soil.

2. A layer of gravel in red clay, about 4′ thick, containing isolated bowlders from a foot to two feet in their longest diameters.

3. About 24′ of a mixed material, consisting mainly of sand and kaolin. The two are sometimes uniformly mixed; at other times they lie in separate masses of two or three feet in thickness at one point, and run down to as many inches at another. In short, the whole bed is a sort of "pell-mell" of sand and clay.

4. A bed of sand, about 10′ thick, generally sharp and clean, but varying from coarse to fine grains, and streaked with iron oxides, with pebbles near bottom of stratum.

5. A thin stratum of clay, about 2′ thick, varying in color from blue to red, and containing in spots fragments of lignite.

6. 2.5′ of sharp, coarse, clean sand.

7. 32.5′ of red clay, mottled with blue and gray, showing no lamination.

8. 5′ of sandy clay, mottled as above. Between this stratum and the clay above, there was no dividing line; the two beds blended gradually along their line of union.

9. A bed of gray, clayey sand, 6′ thick. In this bed occurred, on one side of the shaft, some masses of sandstone, somewhat more ferruginous than the surrounding sand, and on the other side a *tongue* of clean, red clay.

10. A bed of sand with its upper surface horizontal, having a thickness of about 1′ at one side of the shaft and 4′ on the other.

11. A stratum, about 2′ thick, of sandy mud, containing lignite. The laminæ of this bed were horizontal, while its upper surface fell from north to south at the rate of about one in eight.

12. 6′ of sand containing nodules of iron pyrites, isolated masses of lignite, and pockets of red clay.

13. A bed of fine, clean sand, containing here and there a little clay. This bed was 9′ thick, and gradually gave way to the succeeding bed.

14. A bed of sandy kaolin, 6′ thick, very wet and difficult to work. It was a regular mortar-bed in consistence.

15. A layer, 2″ to 4″ thick, of hard, ferruginous conglomerate.

16. 9′ of blue-grey clay, hard, compact, and possessing a very unctuous feel. This bed contained a bunch of rootlets, the first trace of organic remains below the lignite of No. 11.

17. A bed of clayey sand, streaked with red, blue, and grey, 7′ thick, and gradually running into the subjacent stratum.

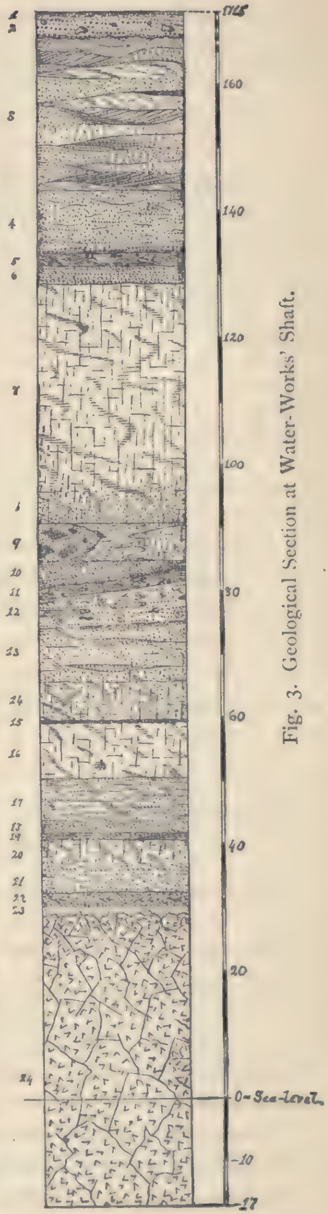

Fig. 3. Geological Section at Water-Works' Shaft.

18. A bed of clean, white, sharp sand, about 2′ thick. (These last nine feet were difficult to work. The material could not be shovelled, and was too sandy to pump.)

19. A layer of red sand about 1′ thick, containing on one side of the shaft a clayey sediment with lignite, and on the other a ferruginous conglomerate.

20. 5′ of blue-black, hard clay, running into a sandy sediment, and this, in turn, into the next stratum.

21. 3.5′ of clean, white sand.

22. 2′ of dark green, compacted sand, containing pebbles and lignite.

23. 1.5′ of fine, sharp sand, almost apple-green in color. Beneath this lay the irregular surface of No. 24.

24. Dark, coarse-grained, soft, chloritic rock. This rock could be easily removed by the pick to a depth of three feet, where blasting was begun at about twenty-six feet above mean tide. The rock grew harder as the depth increased for about ten feet, when it became a chloritic gneiss, and in general remained of that nature through about thirty feet to the bottom of the tunnel grade, or seventeen feet below mean tide.

255TH MEETING. JUNE 7, 1884.

Vice-President BILLINGS in the Chair.

Thirty-five members and guests present.

Mr. G. K. GILBERT presented a

PLAN FOR THE SUBJECT BIBLIOGRAPHY OF NORTH AMERICAN GEOLOGIC LITERATURE.

Mr. J. W. POWELL presented a slightly different plan for the same purpose.

These plans proposed to establish at the outset a limited number of divisions of the subject-matter of the literature and to simultaneously prepare a bibliography of each.

Mr. J. S. BILLINGS criticised the plans at length and advocated that which has been adopted for the indexing of the Library of the Army Medical Museum.

Other remarks were made by Messrs. ANTISELL, NORRIS, GOODE, E. FARQUHAR, F. W. CLARKE, HARKNESS, TONER and WARD.

The meeting announced for October 11 was informally adjourned, to enable members to attend a meeting of the Anthropological Society, and listen to an address by Dr. E. B. Tylor, of Oxford, England.

256TH MEETING. OCTOBER 25, 1884.

The President in the Chair.

Forty members and guests present.

The Chair announced the death, since the last meeting, of Dr. JOSEPH JANVIER WOODWARD, a former President of the Society, Gen. ORVILLE ELIAS BABCOCK, and Gen. BENJAMIN ALVORD.

Announcement was also made of the election to membership of Messrs. WASHINGTON MATTHEWS, STIMSON JOSEPH BROWN, TARLETON HOFFMAN BEAN, and ROBERT EDWARD EARLL.

Mr. S. M. BURNETT read a paper entitled—

ARE THERE SEPARATE CENTRES FOR LIGHT-, FORM-, AND COLOR-PERCEPTION?

controverting the theory which gives an affirmative answer to the question, and maintaining, first, that there is no white-light sensation that cannot be resolved into its constituent elements of color sensation; and, second, that the sense of form is an expression of the idea of extension as represented by the dimensions of the area of the retina impressed. The idea of form is not a purely visual sensation, but is based also on information derived from other sources.

[The paper is published in the *Archives of Medicine*, Vol. XII, No. 2, October, 1884.]

Mr. T. ROBINSON read a paper entitled—

WAS THE EARTHQUAKE OF SEPTEMBER 19TH FELT IN THE DISTRICT OF COLUMBIA?

[Abstract.]

At 3.20 p. m. of September 19 I noticed a peculiar vibration of the floor, table, and chair. I saw my ink shaking and heard the door of the room rattling. The table and chair rocked in a north and south direction. The sounds made by the door were at regular intervals of something less than a second each. My room is on the second floor of the Howard University building.

Immediately after the occurrence I inquired if other persons had noticed anything unusual at that time. One had heard a rumbling, another had felt the shock, and a third had both felt and heard it. The miners in the water-works' tunnel also heard a rumbling noise at about the same hour.

From the motion of my table and chair and the continued thumpings of the door I judge that the shock passed in the direction of the meridian, and continued from ten to fifteen seconds.

There was no local cause for the phenomenon, and I concluded that it was in some way connected with the earthquake that occurred in the West at about the same time.

Mr. PAUL remarked that the direction of the motion communicated to buildings by a slight earthquake shock is not a reliable index of the direction of the earth tremor. The azimuth, amplitude, and period of vibration of the buildings are functions of their structure rather than of the azimuth, amplitude, and period of the earth vibration.

Other remarks were made by Mr. H. A. HAZEN and Mr. ELLIOTT.

Mr. J. S. BILLINGS exhibited a collection of microscopes illustrating the evolution of the mechanical stage. The collection will be sent by the Army Medical Museum to the New Orleans Exhibition.

Mr. BILLINGS read a paper by Mr. WASHINGTON MATTHEWS on

NATURAL NATURALISTS.

[Abstract.]

It is easy to understand that a savage may be well versed in the knowledge of animals and plants which contribute to his wants,

but it is a matter of surprise that with equal care he acquires and disseminates information about creatures which he does not use. I have never yet failed to get from an Indian a good and satisfactory name for any species of mammal, bird or reptile inhabiting his country; and I have found their knowledge of plants equally comprehensive. It is true that not all Indians are equally well informed in this respect, but, as a class, they are incomparably superior to the average white man or to the white man who has not made zoology or botany a subject of study.

There is a prevalent impression that Indians are unable to generalize; and a paragraph goes the rounds of ethnological treatises to the effect that the Chatas have no general term for oak tree, but only specific names for the white oak, the black oak, the red oak, etc. This impression is entirely erroneous. The Indian is as good a generalizer and classifier as his Caucasian brother. His system of classification does not fully coincide with that of the white naturalist, because his system of philosophy leads him to base his groups upon a diffeıent series of resemblances, but his arrangement is nevertheless the result of a process of generalization.

Mr. WARD remarked that his own experience fully sustained the statements of the paper in regard to the botanical ignorance of white men, but less fully in regard to the accuracy of Indian observations. When collecting plants in Utah, he had found that Piute boys and girls gave names to nearly all his specimens, discriminating allied species; but in collating the Indian botanical names recorded by others, he had been led to suspect that certain discrepancies arose from failure to recognize the same species in different stages of development.

Mr. MASON said it is a canon of anthropology that things seem marvellous to us only when we do not understand them, every human phenomenon being governed by law. Our ignorance in regard to wild animals and plants is to be explained by the fact that our activities do not bring us into close relation with them, whereas the savage is dependent on them for sustenance. The market-women who bring herbs to Washington have names for them all, and ignorant mechanics handle technical terms of their craft with great familiarity.

Mr. DUTTON said that his own acquaintance with the Navajos

made him prone to believe that they diagnose species of plants, but he questioned their powers of generalization.

In illustration of Mr. Mason's remark that familiarity is conditioned by contact, he related that rural rambles had made him when a boy so familiar with the fauna and flora of his district that he knew a name for every prominent species. As a man, he had been occupied with other and different matters, and had lost this familiarity.

Mr. WELLING admitted that the Indian was an acute observer, but questioned the propriety of calling him a naturalist. As illustrated by the paper, his methods of interpretation are metaphysical, not scientific.

Other remarks were made by Mr. HILGARD.

257TH MEETING. NOVEMBER 8, 1884.

Vice-President BILLINGS in the Chair.

Forty-eight members and guests present.

Mr. BILLINGS, on behalf of the General Committee, reported the following resolutions:

Resolved, That this Society receives with deep regret the announcement of the the death, on the 17th of August last, of Dr. JOSEPH JANVIER WOODWARD, an ex-president of this Society and one of its original founders.

Resolved, That this untimely death has deprived science of one of its most energetic, patient, and skilful workers and this Society of one of its most efficient and distinguished members.

Resolved, That in our sorrow for this affliction we have some consolation in the knowledge that his long and great suffering is at last ended and that the fruits of his unceasing labors for the last twenty-five years remain for the benefit of the world and as an enduring monument to his memory.

Resolved, That a copy of these resolutions, duly authenticated, be forwarded to his bereaved family.

In presenting these resolutions, Mr. BILLINGS spoke briefly of Dr. Woodward's work and his characteristics as a scientific man,

eulogizing his accuracy of observation, his delicacy of manipulation, his conservatism as a theorist and as a critic of new ideas, and alluding to his delight in teaching and his interest in, and affection for, the Philosophical Society.

Mr. POWELL spoke of his remarkable acumen and his conspicuous mental integrity. Mr. GIHON spoke of his boyhood; Mr. TONER of his ability as a practitioner; and Mr. E. FARQUHAR of the impression of great force conveyed by his presence and conversation.

The resolutions were unanimously adopted.

Mr. C. E. DUTTON made a communication on

THE VOLCANOES AND LAVA FIELDS OF NEW MEXICO,

his remarks being illustrated by photographic lantern views, and by a map exhibiting the boundaries of the region usually termed the Plateau country.

[Abstract.]

Beginning at the north, the boundary of the Plateau country runs along the southern base of the Uinta Range to the junction of the latter with the Wasatch; following the eastern base of the Wasatch southward it strikes off towards the southwestern corner of Utah; thence turning due south it crosses the Colorado river, and gradually shifts its course to the southeastward, preserving this direction for nearly 400 miles and far into New Mexico; here it rapidly turns north northeastward, reaching into the Valley of the Rio Grande, and follows the western bank of that river nearly or quite into Southern Colorado; here the course of the boundary is somewhat indeterminate, but is, in a general way, first northwestward, then northward to the place of beginning. The western and southern border of the Plateau province is usually sharply defined; the plateaus end generally in great cliffs suddenly terminating the horizontal strata, and the profiles drop down upon the rough, irregular topography of a type peculiar to the Great Basin. The eastern border of the Plateau province is by no means so definite; the features peculiar to it pass rather by gradual transition into those characterizing the Rocky Mountains of Colorado.

Among the many geological features of this wonderful region, the volcanic masses are not the least interesting. Volcanic action has prevailed there upon a grand scale, and it may be first noted

that volcanic rocks predominate around the borders of the province. The interior spaces, while not wholly devoid of them, show but a very small amount. The region of the High Plateaus of Utah, which lies upon the western or northwestern border, discloses a very large mass of lavas, erupted chiefly during tertiary time, and representing almost continuous activity from the eocene to the quaternary. Proceeding southward, we are never out of sight of eruptive masses, and in the Unkarets, on the border of the Grand Cañon, we find many scores of old and young cinder-cones and some considerable lava-fields. In the San Francisco Mountains we also have a vast field of volcanic rocks, and thence southeastward they augment in volume and area until at the southernmost extension of the Plateau country they become indeed immense. Still following the boundary northward into the Valley of the Rio Grande they are found abundant, and a singularly interesting field is presented in the neighborhood of Mt. Taylor. The speaker was engaged during the past summer in the geological examination of the Mt. Taylor district, and it is of the striking features there presented that he designs especially to speak.

Mt. Taylor is an old volcano long since extinct. Its altitude is about 11,400 feet above the sea. It stands upon a high *mesa*, from the summit of which it rises as an ordinary volcanic cone of considerable magnitude—much larger than Vesuvius, much smaller than Ætna. Its lavas are rather monotonous in type, so far as external appearances are concerned, consisting probably of basalts and andesites. The *mesa* upon which it stands is of great extent, being 40 miles long and 25 miles wide. It is composed of nearly horizontal cretaceous strata, capped everywhere with basalt or andesite, ranging from 200 to 400 feet in thickness. To the northeast and to the south of it are similar high *mesas*, also capped by basalt and andesite, but presenting no great volcanic pile like Mt. Taylor. The only features which indicate volcanic vents are barely noticeable hillocks, which scarcely affect the evenness of the horizontal surfaces and which are wholly incommensurate, apparently, with the vast lava caps upon which they occur.

These lavas are all of tertiary age. It would be difficult to say to what divisions of tertiary time their activity should be assigned, but it cannot have been very late tertiary and it is reasonably certain that it cannot have been very old tertiary. In a general

way their activity is inferred to have prevailed in a period not far from middle tertiary time—possibly in the miocene. The large amount of erosion which has occurred since their eruptions ceased forbids a much later period, and the still larger amount of tertiary erosion which preceded this activity equally forbids a much earlier one.

Upon the summits of the *mesas* no recent eruptive rocks occur. But in the broad valleys which lie between them and around them are lavas of quite another age. These valley lavas are all recent. Indeed the most superficial observer is at once impressed with the freshness of their aspect, and critical examination confirms the view that none of them have any geologic antiquity, while some of them are so modern that it seems as if half a dozen centuries were a large estimate of the time which separates us from their outflow.

These recent eruptions are basalts of normal type. The external aspects of the fields of young lava resemble those of the Hawaiian Islands. The two forms of solidified lava are well presented, viz: the viscous or ropy, and the rough clinker fields.

A striking characteristic of both old and young lavas—those upon the *mesa* summits and those in the valleys below—is the usual though not universal absence of cinder cones or piles of fragmental matter built up around the orifices from which the lavas were extruded. The eruptions, with the exception of those of Mt. Taylor, belonged to the quiet order which are typified among volcanoes now active, by Mauna Loa and Kilauea.

But the volcanic remnants which appeal most strongly to the imagination of the observer, remain to be described. In the broad valleys which separate the lava-capped *mesas* are seen many conspicuous objects rising as sharp peaks or aiguilles of rock to great altitudes. They are very black in color, and contrast powerfully with the bright tints of the sedimentary beds around them. These peaks, which range in altitude above the valley plains from 700 or 800 feet to 2;000 feet, consist of columnar basalt. They are, in fact, the ancient lavas which congealed in the volcanic pipes, while the sedimentary strata which formerly inclosed them have been swept away in the great erosion of the country. In that long-continued and great denudation these "necks," by their more adamantine character, have resisted the general decay, and remain to attest the former extension of the strata over the valleys and the

existence, prior to their denudation, of volcanic extravasations which probably covered them wholly or in part. In the *mesa* walls and on their slopes may be seen numerous instances of partially excavated necks, while in others the necks are just beginning to be exhumed. In the latter cases remnants of the old cinder-cones which were piled up over their summits are still preserved, so that natural sections of the whole apparatus are exhibited. There are many scores of these necks, and the effects of erosion in unearthing them are exhibited in all stages. Wherever the true neck or core is disclosed the basalt is seen to be columnar, and the columns are often arranged in beautiful fashions.

No more striking illustration and proof of a great erosion could be mentioned than is here disclosed, and the region must become a classic one, to be referred to by future geologists as an excellent example of some of the grandest laws and processes with which their science deals.

Mr. POWELL spoke of the distribution of eruptions. They are apt to occur on the faces of acclivities undergoing erosion, but not on acclivities due to displacement. Near a fault they break through the uplifted block rather than the thrown. They do not occur in the bottoms of cañons.

In mapping the Plateaus he had thrown the boundary farther north than Captain Dutton, so as to include a large area north of the Uinta Mountains.

The peculiarly favorable conditions under which geology is studied in the plateau region enable its features to be comprehended without the doubts and the laborious compilation of details elsewhere necessary. It results that while the structure of the Plateau country is as well known as that of any equal area in the world, the literature of its geology is exceedingly small.

Other remarks were made by Messrs. WHITE and GILBERT.

258TH MEETING. NOVEMBER 22, 1884.

The President in the Chair.

Forty-nine members and guests present.

Mr. E. B. ELLIOTT made a communication on

ELECTRIC LIGHTING,

which was discussed by Messrs. HILGARD, WELLING, MUSSEY, PAUL, and POWELL.

Mr. H. ALLEN HAZEN made a communication on

THERMOMETER EXPOSURE.

[Abstract.]

In recent experiments for determining the relative values of temperatures in city and country, it has been found that ordinarily, on clear days, in the early morning, at 6 feet above ground, in the country, temperatures are 4 to 5 degrees lower than in the city, and also that the air is always nearly saturated in the country, but not as nearly in the city. This is due more to intense radiation from grass in the country, this cooling the air to the dew point, than to the heating and drying from pavements and walls or chimneys of houses.

To obtain a standard air temperature it is proposed to use bright and black bulb thermometers joined together and swung over grass ground under an umbrella, with no shade from trees or buildings, in the day time. Under such circumstances the two thermometers can be brought within 0.5° of each other, and the true air temperature may be taken as about as much lower than the bright-bulb as that is lower than the black.

Recent experiments with six different thermometer shelters indicate a general agreement, except in the case of the Wild shelter. The peculiar condition effected by the Wild shelter is inferior ventilation, and the experiments indicate the practical sufficiency of the single-louvred shelter. To determine the humidity with the psychrometer in still air, the employment of artificial ventilation is recommended.

Remarks were made by Mr. PAUL.

259TH MEETING. DECEMBER 6, 1884.

By courtesy of the officers of the Columbian University, the meeting was held in the lecture hall of the University building.

Members of the Anthropological, Biological, and Chemical Societies and their friends were present by invitation.

Mr. J. W. Powell, by request of the President, occupied the Chair.

Present, one hundred and four members and guests.

The business of the evening was the presentation of the Annual Address of the President, Mr. J. C. Welling. In introducing him to the audience, the Chairman sketched the history of the Society, describing the socio-scientific club of which it was the offspring, and referring to the younger scientific societies of Washington, of which it might be regarded as the parent.

The President then read an address on

THE ATOMIC PHILOSOPHY, PHYSICAL AND METAPHYSICAL.

[Printed in full on pp. xxix–lix.]

On motion of Mr. Gregory, the Society tendered its President a vote of thanks for his efficient administration and instructive address.

260th Meeting. December 20, 1884.

THE FOURTEENTH ANNUAL MEETING.

The President in the chair.

The Chair announced the death, since the last meeting, of Mr. Henry Wayne Blair.

The Chair announced the election to membership of Mr. Robert Edwards Carter Stearns.

It was announced that the Mathematical Section would, in the future, hold its meetings in the mathematical class room of the Columbian University, the use of that room having been tendered by the officers of the University.

The order of business was then read, and afterward the minutes of the last annual meeting.

The report of the Secretaries were read and accepted. (Printed on page XXIII.)

The report of the Treasurer was read, received, and referred to an Auditing Committee, consisting of Messrs. H. C. Yarrow, Marcus Baker, and W. C. Winlock. (The report is printed on pages XXIV and XXV.)

The minutes of the 258th and 259th meetings were read and approved.

The officers of the ensuing year were then elected. (The list is printed on page XV.)

The rough minutes of the meeting were read, and the meeting adjourned.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

MATHEMATICAL SECTION.

STANDING RULES

OF THE

MATHEMATICAL SECTION.

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1. The object of this Section is the consideration and discussion of papers relating to pure or applied mathematics.

2. The special officers of the Section shall be a Chairman and a Secretary, who shall be elected at the first meeting of the Section in each year, and discharge the duties usually attaching to those offices.

3. To bring a paper regularly before the Section it must be submitted to the Standing Committee on Communications for the stated meetings of the Society, with the statement that it is for the Mathematical Section.

4. Meetings shall be called by the Standing Committee on Communications whenever the extent or importance of the papers submitted and approved appear to justify it.

5. All members of the Philosophical Society who wish to do so may take part in the meetings of this Section.

6. To every member who shall have notified the Secretary of the General Committee of his desire to receive them, announcements of the meetings of the Section shall be sent by mail.

7. The Section shall have power to adopt such rules of procedure as it may find expedient.

OFFICERS

OF THE

MATHEMATICAL SECTION FOR 1884.

Chairman, ASAPH HALL. *Secretary*, HENRY FARQUHAR.

LIST OF MEMBERS WHO RECEIVE ANNOUNCEMENT OF THE MEETINGS.

ABBE, C.	HALL A.
AVERY, R. S.	HARKNESS, W.
BAKER, M.	HAZEN, H. A.
BATES, H. H.	HILGARD, J. E.
BILLINGS, J. S.	HILL, G. W.
BURGESS, E. S.	KING, A. F. A.
CHRISTIE, A. S.	KUMMELL, C. H.
COFFIN, J. H. C.	MCGEE, W J
CURTIS, G. E.	NEWCOMB, S.
DELAND, T. L.	PAUL, H. M.
DOOLITTLE, M. H.	LEFAVOUR, E. B.
EASTMAN, J. R.	PEIRCE, C. S.
EIMBECK, W.	RITTER, W. F. M'K.
ELLIOTT, E. B.	SMILEY, C. W.
FARQUHAR, H.	TAYLOR, W. B.
FLINT, A. S.	UPTON, W. W.
GILBERT, G. K.	WALLING, H. F.
GORE, J. H.	WINLOCK, W. C.
GREEN, B. R.	WOODWARD, R. S.

BULLETIN

OF THE

MATHEMATICAL SECTION.

10TH MEETING. JANUARY 30, 1884.

The Chairman presided.

Seventeen members and guests present.

The Section proceeded, under Rule 2, to the election of a Chairman and a Secretary for the year 1884. On motion of Mr. ELLIOTT, the rules governing the elections of the Society were adopted. The officers for 1883—Mr. HALL, as Chairman, and Mr. H. FARQUHAR, as Secretary—were re-elected, after each had briefly expressed a desire that the choice might fall on some one else.

Mr. KUMMELL read an extract from a letter lately received from Mr. Artemas Martin, of Erie, Pennsylvania, in which the formation of an American Mathematical Society was recommended. After some informal discussion, Mr. WINLOCK moved the appointment of a special committee, with instructions to report on the advisability of taking steps for the formation of such a society. On motion of Mr. ELLIOTT, the matter was postponed.

Mr. KUMMELL then made a communication on

CURVES SIMILAR TO THEIR EVOLUTES,

in which he made use of the intrinsic equation, and showed this property to belong to a whole class, of which the logarithmic spiral is at one extreme and the cycloids are at the other.*

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* Prof. Benjamin Peirce solved a problem almost identical with this one, in *Gill's Mathematical Miscellany* for May, 1839, by essentially the same methods. This solution, which had not been seen by Mr. Kummell at the time of reading his paper, is believed to contain the first use of what has since become known as the "intrinsic equation."

Remarks on this communication were made by Messrs. CHRISTIE and HILL.

Mr. G. K. GILBERT made a communication on

THE PROBLEM OF THE KNIGHT'S TOUR.

[Abstract.]

The ordinary problem, requiring the knight to traverse the chess-board and return to his original position in sixty-four moves, is susceptible of very numerous solutions, and is not difficult. Its interest is increased by extending it so as to include fields of other form and size.

It is readily shown that a perfect tour is impossible on any field containing an odd number of squares.

A *symmetric* tour is one divisible into two or more similar parts. A tour has *bilateral symmetry* when one-half, being turned face downward upon the other, coincides with it. A tour has *biradial symmetry* when one-half, being rotated through 180° about the center of figure, coincides with the other half. A tour has *quadri-radial symmetry* when its fourth part, being rotated through 90° about the center of figure, coincides with the adjacent quarter.

A tour having bilateral symmetry cannot be devised on a field containing a number of squares divisible by four.

A tour having biradial symmetry cannot be devised on a field whose number of squares is divisible by two and not by four.

A tour having quadriradial symmetry cannot be devised on a field whose number of squares is divisible by eight.

It follows that *on square fields* the tour is impossible if the number of spots on a side is odd; bilateral symmetry is never possible; quadri-radial symmetry is possible only when the number of squares on a side is the double of an odd number. The only symmetry possible on a chess-board is biradial.

The above conclusions are deductive. It is determined empirically that the smallest square field on which the tour can be executed is that with 36 spots. Upon this field the number of possible tours with biradial symmetry is twenty-one, of which five have also quadriradial symmetry.

Remarks on this communication were made by Messrs. ELLIOTT and HALL, who called attention to previous work on the subject.

11TH MEETING. FEBRUARY 20, 1884.

The Chairman presided.

Eighteen members and guests present.

Mr. H. FARQUHAR made a communication on

EMPIRICAL FORMULÆ FOR THE DIMINUTION OF AMPLITUDE OF A FREELY-OSCILLATING PENDULUM.

[Abstract.]

The theoretical formulæ usually employed are obtained by integration from an expression for the diminution of the amplitude in terms of the amplitude itself. The most important term in this expression is one involving the first power of the amplitude, indicating a resistance proportional to the velocity of the pendulum's motion. A term containing the square of the velocity (or amplitude) also enters; and, to allow for the friction of the pendulum knife-edge on its support, a term independent of the velocity would have to be added. Atmospheric resistance to very high velocities is found, moreover, to be proportional to a higher power than the square of the velocity. There are thus more than three terms theoretically required to express the resistance, and these must be calculated, such is the uncertainty of the subject and the complexity of the conditions on which the different resistances depend, from the observations themselves. Since these observations must also be depended on for an additional constant (the amplitude at some initial time or the time of some standard amplitude), and since they are not complete or exact enough to furnish more than three constants, or four in a few exceptional cases, it is obvious that a good approximation to theory must content us in practice.

Two convenient methods of representing amplitude in terms of time are suggested by imposing arbitrary conditions. First, taking three terms to express the diminution (the amplitude being φ), thus:

$$a + b\varphi + c\varphi^2,$$

suppose the square of half the middle co-efficient equal to the product of the other two. This expression has then the form:

$$\frac{1}{a}(\varphi + b)^2.$$

Integrating this value of $-D_t\varphi$, and supplying a constant, we have:

$$(\varphi + b)(t - e) = a,$$

in which the constants $a + be$, e and $-b$, are easy to calculate by least squares.

To show the agreement of this formula with observation, take Mr. Pierce's "mean swing" at three European stations (U. S. Coast Survey Report for 1876, appendix 15, pages 232, 271) and apply $b = 29'.2$, $e = -7632^s$, $a = 756847$, in calculating φ from t. Hence the following table:

t.	φ, obs'd.	φ, calc'd.	Residuals (1st).	Residuals (2d).
-2880^s	130′	130′.07	−0′.07	−0′.16
−2187	110	109.80	+0.20	+0.13
−1779	100	100.11	−0.11	−0.04
−706	80	80.08	−0.08	+0.24
0	70	69.97	+0.03	+0.40
+1927	50	49.98	+0.02	0.00
+3304	40	40.01	−0.01	−0.66

The agreement (in column "residuals, 1st") is as close as could be desired. The equation is that of the equilateral hyperbola, with asymptotes parallel to the axes of φ and t. This agreement can be made still closer by inclining one of the asymptotes, a term $-c(t-e)^2$ being added. There are thus four constants to compute; but this form of equation has the advantage of having its constants directly deducible by least square reduction. With the additional term, a perfect agreement between theory and the most precise observations hitherto made can be attained. As an instance, the thirty-five observations of amplitude, from over 2° down to 10′, given by Prof. Oppolzer in the Proceedings of the Vienna Academy for October, 1882, were compared with the formula

$$(\varphi + 60'.6)(t + 10.8) - 0.5\,(t + 10.8)^2 = 2178.1$$

(the unit of t being an interval of about $5^m.7$) and of the residuals, which need not be given in detail, the largest was 0′.8. A similar accordance was found in a set of observations extending over six hours, the pendulum swinging under less than half an inch of atmospheric pressure. (See Mr. Pierce's report, page 248, last two columns combined.) In this formula,

$$D_t \varphi = -\frac{1}{2a}\left\{(\varphi+b)^2 - 4ac + (\varphi+b)\sqrt{(\varphi+b)^2 - 4ac}\right\}$$
$$= -\frac{1}{a}(\varphi+b)^2 + 3c + \frac{ac^2}{(\varphi+b)^2} + \text{etc.}$$

The correction to the time of oscillation $\left(\frac{1}{16} D_t^{-1} \varphi^2\right)$ involves the logarithm of $t - e$, and is not very simple in practical application.

The second convenient method is the one by which the residuals in the last column of the table above given were calculated. In this the rate of diminution is supposed proportional to φ^{1+n}, n being a proper fraction. Hence,

$$\varphi^n (t-e) = a, \text{ and } D_t^{-1} \varphi^2 = -\frac{na^{\frac{2}{n}}}{(2-n)(t-e)^{\frac{2}{n}-1}} = -\frac{na\varphi^{2-n}}{2-n}$$

This formula is very simple, and the table shows its agreement with observation to be fair for the larger amplitudes—those of chief importance. In this calculation $n = \frac{1}{2}$, $e = -10716^s$, and $a = 89400$. Better results would have been obtained by using a slightly smaller value of n, say 0.44; but in practice the nearest tenth or reciprocal of a whole number is sufficient. In reducing the observations given by Prof. Oppolzer, n was taken equal to 0.28; but one of the residuals exceeded 1′, though two others were as high as 0′.9. The observations at low pressures, above referred to, indicated a much smaller n. By using the value 0.04, however, the agreement of formula and observation was perfect. n thus appears to be nearly proportional to the square root of the atmospheric pressure; but when very small, it may be supposed to vanish, and φ^n replaced by the logarithm of φ. In this case e will of course be the time of unit-amplitude, instead of that of infinite amplitude as in former cases.

No two observations of the diminution of amplitude of the same pendulum will in general be found to be copies of each other, for differences in atmospheric conditions and in friction on the support, imperceptible otherwise, will manifest themselves in a changed rate of diminution. Even in calculating the correction for different parts of one extended swing, it is advisable to adopt different values of one or other of the constants found. By so varying the quan-

tity e, in the formula last given, all disadvantages from its want of exact accordance with observation disappear, and the results are brought far within the needful limits of accuracy.

Mr. GILBERT then stated

A CONCRETE PROBLEM IN HYDROSTATICS,

suggested by the fact that the shore-line of a quaternary lake in the Great Basin is shown by levels to be more than a hundred feet higher on elevated land, that once formed islands near its middle part, than on the margin of the lake. This inland sea, known as Lake Bonneville, was one hundred and twenty miles across. Among the possible explanations of the present difference of level, the effect of the removal of a large body of water in changing the form of level surfaces in its basin had been suggested, and the problem was to find how great an effect was due to this cause.

In the discussion that followed, Mr. PAUL called attention to the complexity of the calculation of equipotential surfaces.

Mr. WOODWARD had formerly made a somewhat similar computation to ascertain the deflection of the plumb-line caused by unequal local attraction to eastward and to westward at the eastern end of Lake Ontario; from which it appeared to result that the effect due to this cause was insignificant in comparison with that required by the problem.

Other remarks were made by Messrs. DOOLITTLE, HILL, H. FARQUHAR, and S. J. BROWN.

At the request of the Chairman, a communication promised by him was postponed until next meeting.

12TH MEETING. MARCH 5, 1884.

The Chairman presided.

Fifteen members present.

Mr. A. HALL read the following paper on

THE FORMULÆ FOR COMPUTING THE POSITION OF A SATELLITE.

The method of rectangular co-ordinates in space furnishes a very simple and at the same time a general method of treating many questions in astronomy. This method was introduced into practical astronomy by Lagrange in his memoir on the Transit of Venus, June 3, 1769 (Berlin Academy Memoirs, 1766). Whenever we have to consider the relations of three points in space, we may take the origin of co-ordinates at one of the points, and then forming the values of the rectangular co-ordinates of the other points in terms of the polar co-ordinates, the sum or difference of two of the x co-ordinates being equal to the third x co-ordinate, we have an equation between the three polar co-ordinates. Similar relations hold for the axes of y and z, and hence result three equations between the two angles and the distance that are required to be found. This method is extremely useful, and can be applied to a great number of questions in parallax, aberration, eclipses, and to those that occur in nearly every part of spherical astronomy. A great recommendation of this method is its simplicity, and the fact that it is so closely connected with first principles that it can be applied with the greatest ease. After the equations are formed they have only to be transformed by known rules, and the whole work is thus reduced to algebraic and trigonometric transformations which can be safely made. These advantages are so great that it is not surprising that this method of treating astronomical questions has come so largely into use, and the generality and elegance of the process are in marked contrast with the old methods which proceed by spherical trigonometry. Perhaps a disadvantage of the new method is that it is too mechanical, and one is apt to forget or never know the meaning of the quantities that are employed. The old geometrical methods have therefore their value in calling to mind a more exact knowledge of the quantities that are used in the solution of a problem.

In the method which Bessel has employed for computing the position of a satellite, he has derived his formulæ by Lagrange's method. Thus if a and δ be the apparent right ascension and declination of the planet at any instant, a', δ' the same quantities for the satellite, and if ρ and ρ' be their distances from the earth, and if r be the radius vector of the satellite, and a and d its right ascension and declination seen from the planet, we have, by the method of rectangular co-ordinates,

$$\begin{aligned}
\rho' \cos \delta' \cos a' &= \rho \cos \delta \cos a + r \cos d \cos a \\
\rho' \cos \delta' \sin a' &= \rho \cos \delta \sin a + r \cos d \sin a \qquad (1) \\
\rho' \sin \delta' &= \rho \sin \delta + r \sin d
\end{aligned}$$

If p and s are the angle of position and distance of the satellite with respect to the center of the planet, the spherical triangle formed by the pole of the equator, the planet, and the satellite gives us the following equations:

$$\begin{aligned}
\cos s &= \sin \delta \sin \delta' + \cos \delta \cos \delta' \cos (a' - a) \\
\sin s \cos p &= \cos \delta \sin \delta' - \sin \delta \cos \delta' \cos (a' - a) \qquad (2) \\
\sin s \sin p &= \cos \delta' \sin (a' - a)
\end{aligned}$$

If N and J be the longitude of the node of the orbit of the satellite on the equator, and its inclination to the equator, and u the distance of the satellite from the node counted on its orbit, we have

$$\begin{aligned}
\cos d \sin (a - N) &= \sin u \cos J \\
\cos d \cos (a - N) &= \cos u \qquad (3) \\
\sin d &= \sin u \sin J
\end{aligned}$$

These three sets of equations are fundamental, and are sufficient for the complete solution of the problem—Given the orbit of a satellite to determine its apparent angle of position and distance. We have only to transform these equations, and, in order to ease the computation, to introduce, as Bessel has done, certain auxiliary quantities which depend on the position of the planet in the heavens, and the position of the orbit of the satellite with respect to the equator. These auxiliary quantities will of course vary with the position of the planet, and also from the slow changes that the node and inclination of the orbit undergo, but they can be tabulated easily. So far therefore, as the practical solution of this question is concerned there is not much more to be desired, but it is interest-

ing to look at the problem from another point of view, and one that will lead us to consider more closely its geometry.

Imagine a set of rectangular axes in space, the origin being at the center of the planet, and denote by X, Y, Z the points on the celestial sphere made by the intersections of these axes. Let S be the point where the prolongation of the radius vector of the satellite strikes the sphere; then we have for the co-ordinates of the satellite

$$x = r.\cos SX$$
$$y = r.\cos SY$$
$$z = r.\cos SZ$$

We can express these cosines by means of six auxiliary quantities similar to those that Gauss has used for computing the position of a planet. Take the prolongation of the right line drawn from the earth to the planet as the axis of Z, the axis of Y in the plane of the declination circle that passes through Z, and the axis of X at right angles to this plane and in the direction of increasing right ascensions. Let O be the pole of the equator and T the positive pole of the orbit of the satellite. Introduce the following notation, which is the same as Bessel's:

$$\text{arc } TX = f, \quad \text{angle } OTX = F$$
$$\text{" } TY = g, \quad \text{" } OTY = G$$
$$\text{" } TZ = h, \quad \text{" } OTZ = H$$

Since the arc $TS = 90°$, the spherical triangles STX, STY, and STZ give

$$\cos SX = \sin f \cos STX$$
$$\cos SY = \sin g \cos STY$$
$$\cos SZ = \sin h \cos STZ$$

The distance of the satellite in its orbit from the node being u, and the angle OTN being 90°, we have

$$STX = 90° - (F + u)$$
$$STY = 90° - (G + u)$$
$$STZ = 90° - (H + u)$$

And the values of the co-ordinates are therefore:

$$\begin{aligned} x &= r.\sin f \sin (F + u) \\ y &= r.\sin g \sin (G + u) \\ z &= r.\sin h \sin (H + u) \end{aligned} \qquad (4)$$

These are the values at which Bessel arrives by the analytical method. The arcs f, g, h are always less than 180°, and the only difficulty is in counting the angles F, G, H. In the purely analytical process we merely substitute so as to satisfy the equations, and the result is right if we pay attention to the algebraic signs; but in the preceding quasi geometrical method we must be careful to count the angles F, G, H in the direction of increasing right ascensions from 0° to 360°. The formulæ for computing the six auxiliary quantities can be found from the spherical triangles TOX, TOY, TOZ. In these triangles the angles at O are

$$\begin{aligned} TOX &= 180° - (\alpha - N) \\ TOY &= \ \ 90 - (\alpha - N) \\ TOZ &= \ \ 90 + (\alpha - N) \end{aligned}$$

Hence, we have

$$\begin{aligned} \cos\ f &= -\sin J \cos (\alpha - N) \\ \sin f \sin F &= -\sin (\alpha - N) \\ \sin f \cos F &= \cos J \cos (\alpha - N) \\ \cos\ g &= \cos \delta \cos J + \sin \delta \sin J \sin (\alpha - N) \\ \sin g \sin G &= -\sin \delta \cos (\alpha - N) \\ \sin g \cos G &= \cos \delta \sin J - \sin \delta \cos J \sin (\alpha - N) \\ \cos\ h &= \sin \delta \cos J - \cos \delta \sin J \sin (\alpha - N) \\ \sin h \sin H &= \cos \delta \cos (\alpha - N) \\ \sin h \cos H &= \sin \delta \sin J + \cos \delta \cos J \sin (\alpha - N) \end{aligned} \qquad (5)$$

The computation of these formulæ may be changed by introducing other auxiliary quantities, as is commonly done, but nothing is gained by such a change if the computer is accustomed to the use of addition and subtraction logarithms.

By means of the spherical triangles we can find a number of elegant relations among the quantities f, g, h, F, G, H. But we have first

$$\cos f^2 + \cos g^2 + \cos h^2 = 1,$$

or these are the direction cosines of the line drawn from the planet to the pole of the orbit of the satellite.

The triangle XTY gives

$$\cos XY = \cos XT \cos YT + \sin XT \sin YT \cos XTY,$$

and we have $\qquad XY = 90°, XTY = F - G,$

hence the values of $\cos XY$, $\cos YZ$, $\cos ZX$ furnish the equations

$$\begin{aligned} \cos(F-G) &= -\operatorname{cotg} f \operatorname{cotg} g \\ \cos(G-H) &= -\operatorname{cotg} g \operatorname{cotg} h \\ \cos(H-F) &= -\operatorname{cotg} h \operatorname{cotg} f \end{aligned} \qquad (6)$$

Again the triangle XTY gives

$$\cos f = \sin g \cos TYX,$$

and from the triangle TYX

$$\sin h \sin YTZ = \sin TYZ,$$

but $$TYX - TYZ = 90°,$$

and $$YTZ = -(G-H),$$

hence these equations and similar ones give

$$\begin{aligned} \sin(F-G) &= \frac{\cos h}{\sin f \sin g} \\ \sin(G-H) &= \frac{\cos f}{\sin g \sin h} \\ \sin(H-F) &= \frac{\cos g}{\sin h \sin f} \end{aligned} \qquad (7)$$

By combining equations (6) and (7), we have

$$\operatorname{cotang}(F-G) = -\frac{\cos f \cos g}{\cos h}$$

$$\operatorname{cotang}(G-H) = -\frac{\cos g \cos h}{\cos f}$$

$$\operatorname{cotang}(H-F) = -\frac{\cos h \cos f}{\cos g}$$

$$\cos f^2 = \operatorname{cotg}(F-G)\operatorname{cotg}(H-F)$$
$$\cos g^2 = \operatorname{cotg}(G-H)\operatorname{cotg}(F-G)$$
$$\cos h^2 = \operatorname{cotg}(H-F)\operatorname{cotg}(G-H)$$

$$\sin f^2 = -\frac{\cos(G-H)}{\sin(F-G)\sin(H-F)}$$

$$\sin g^2 = -\frac{\cos(H-F)}{\sin(G-H)\sin(F-G)}$$

$$\sin h^2 = -\frac{\cos(F-G)}{\sin(H-F)\sin(G-H)}$$

These six auxiliary quantities are therefore strictly analogous to those which Gauss introduced for computing the position of a planet. For controlling the computation, we have

$$\tang J = \frac{\sin g \sin h \sin (H - G)}{\sin f \cos F},$$

an equation in which each of the six auxiliaries enters into the value of J.

If we introduce another auxiliary quantity, and put the angle

$$TZO = 180^\circ - k,$$

it follows, from the manner adopted for counting an angle of position, that

$$TZO = 180^\circ - (p - k).$$

Denoting the angle between the radius vector and the axis of Z by σ, the spherical triangle TZS gives

$$\begin{aligned} \sin \sigma \sin (p - k) &= \cos (H + u) \\ \sin \sigma \cos (p - k) &= \sin (H + u) \cos h \\ \cos \sigma &= \sin (H + u) \sin h \end{aligned} \tag{8}$$

But we have also

$$\begin{aligned} \rho' \sin s &= r \sin \sigma \\ \rho' \cos s &= r \cos \sigma + \rho, \end{aligned}$$

and by uniting these equations with (8), we can find s and p. This method of finding the distance and the angle of position is due to Marth, and as it is in constant use by him for the very convenient ephemerides of satellites which he publishes, it may be well to consider it further. If we multiply equations (8) by r, and then substitute the values of $r \sin \sigma$ and $r \cos \sigma$ from the last equations, we have

$$\begin{aligned} \rho' \sin s \sin (p - k) &= r \cos (H + u) \\ \rho' \sin s \cos (p - k) &= r \sin (H + u) \cos h \\ \rho' \cos s &= r \sin (H + u) \sin h + \rho \end{aligned} \tag{9}$$

Instead of these exact equations we may use in nearly all known cases of satellites the first two equations and put ρ for ρ' and s for $\sin s$. The equations for use are then

$$\begin{aligned} s \sin (p - k) &= \frac{r}{\rho} \cos (H + u) \\ s \cos (p - k) &= \frac{r}{\rho} \sin (H + u) \cos h \end{aligned} \tag{10}$$

If we express s and r in seconds of arc, and assume that the orbit is circular, $\frac{r}{\rho}$ will be the semi-major axis of the apparent ellipse described by the satellite, and $\frac{r}{\rho}\cos h$ will be the semi-minor axis. The quantities $\frac{r}{\rho}$, $\frac{r}{\rho}\cos h$, H and k can be tabulated, and equations (10) furnish the easy method of computing s and p which is employed by Marth (Monthly Notices, Royal Astronomical Society.)

For computing k we have from the triangle TZO

$$\begin{aligned} \sin h \sin k &= \cos(\alpha - N)\sin J \\ \sin h \cos k &= -\sin(\alpha - N)\sin J \sin\delta - \cos J \cos\delta \end{aligned} \qquad (11)$$

and, also,

$$\begin{aligned} \sin h \sin k &= -\cos f \\ \sin h \cos k &= -\cos g \end{aligned}$$

In what precedes it is assumed that the orbit of the satellite is known. If this orbit is not known the easiest method of proceeding seems to be the following: First, we assume the orbit of the satellite to be a circle, and from the observed angles of position and the observed distances determine the major and minor axes of the apparent ellipse described by the satellite around the planet, and the angle of position of the minor axis. Generally these quantities can be found by a graphical method. The preceding angle k is the angle of position of the minor axis, and $\cos h$ is found from the ratio of the two axes. Then from the triangle TOZ we have the equations

$$\begin{aligned} \sin J \cos(N - \alpha) &= \sin h \sin k \\ \sin J \sin(N - \alpha) &= \cos h \cos\delta + \sin h \sin\delta \cos k \\ \cos J &= \cos h \sin\delta - \sin h \cos\delta \cos k \end{aligned} \qquad (12)$$

With the approximate values of J and N found from these equations we can compute the auxiliary quantities depending on the position of the plane of the orbit and the position of the planet, and can determine the elements belonging to the plane of the orbit. These approximate elements can afterwards be corrected by equations of condition or by other methods.

In work of this kind it is more convenient to have the inclination and node of the orbit referred to the equator, and since these elements are commonly given with respect to the ecliptic we have to transfer them to the equator. If n and i are the node and inclina-

tion referred to the ecliptic, ε the obliquity of the equator, and w the distance from the ecliptic to the equator counted on the orbit. we have the following equations for finding J, N, and w. These equations come from the triangle between the equator, the ecliptic, and the orbit of the satellite. They are similar to those given in the Theoria Mot., Art. 55,

$$\sin \tfrac{1}{2} J \cos \frac{w-N}{2} = \cos \frac{n}{2} \sin \frac{\varepsilon + i}{2}$$

$$\sin \tfrac{1}{2} J \sin \frac{w-N}{2} = \sin \frac{n}{2} \sin \frac{\varepsilon - i}{2}$$

$$\cos \tfrac{1}{2} J \cos \frac{w+N}{2} = \cos \frac{n}{2} \cos \frac{\varepsilon + i}{2}$$

$$\cos \tfrac{1}{2} J \sin \frac{w+N}{2} = \sin \frac{n}{2} \cos \frac{\varepsilon - i}{2}$$

For the inverse problem of finding i, N, and w from J, N, and ε, we have from the same triangle

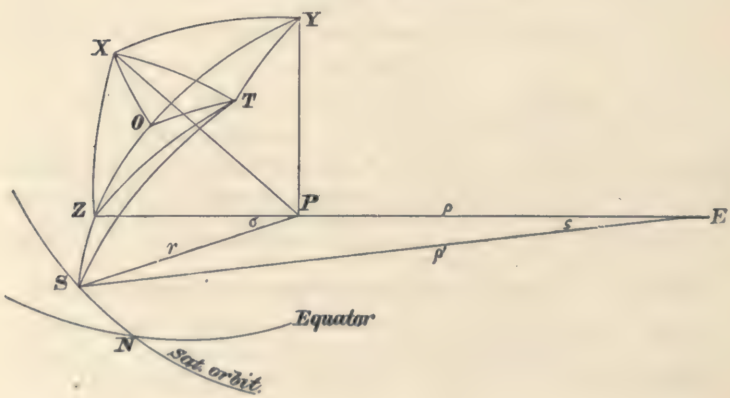
$$\cos \tfrac{1}{2} i \cos \frac{n-w}{2} = \cos \frac{N}{2} \cos \frac{J-\varepsilon}{2}$$

$$\cos \tfrac{1}{2} i \sin \frac{n-w}{2} = \sin \frac{N}{2} \cos \frac{J+\varepsilon}{2}$$

$$\sin \tfrac{1}{2} i \cos \frac{n+w}{2} = \cos \frac{N}{2} \sin \frac{J-\varepsilon}{2}$$

$$\sin \tfrac{1}{2} i \sin \frac{n+w}{2} = \sin \frac{N}{2} \sin \frac{J+\varepsilon}{2}$$

POSITION OF A SATELLITE.



$TX = f,\ OTX = F,\ OT = J, \qquad STZ = 90° - (H + u)$
$TY = g,\ OTY = G,\ OY = \delta, \qquad TZO = 180° - k$
$TZ = h,\ OTZ = H,\ OZ = 90° - \delta,\ TZS = 180° - (p - k)$
$NS = u,\ NOZ = a - N,\ TOZ = 90° + (a - N),\ SZO = 360° - p$

$$TOX = TOY + 90 = 180° - (a - N)$$

N is the pole of OT, $\therefore NOT = NTO = 90°$

In response to a question, Mr. HALL said that in computations of orbits of double stars, as little reliance should be placed upon measures of distance as possible. Variations of angular velocity are far safer.

Mr. G. W. HILL made a communication on

A FORMULA FOR THE LENGTH OF A SECONDS-PENDULUM,

which is published in full in the Astronomical Papers of the American Ephemeris, Vol. III, Part 2, Chapter V.

13TH MEETING. MARCH 26, 1884.

The Chairman presided.

Fourteen members present.

Mr. ALEX. S. CHRISTIE made a communication on

A FORM OF THE MULTINOMIAL THEOREM.

This communication is reserved by the author. Remarks were made by Mr. HILL.

Mr. R. S. WOODWARD gave a

DISCUSSION OF A CONCRETE PROBLEM IN HYDROSTATICS PROPOSED BY MR. G. K. GILBERT.

Remarks on this communication were made by Mr. GILBERT.

Mr. C. H. KUMMELL gave the first part of a communication on

THE QUADRIC TRANSFORMATION OF ELLIPTIC INTEGRALS,

which was unfinished when the hour of adjournment arrived.

14TH MEETING. MAY 7, 1884.

The Chairman presided.

Nine members present.

In the absence of the Secretary, the minutes were read by Mr. CHRISTIE.

Mr. KUMMELL finished the paper begun by him at last meeting on

THE QUADRIC TRANSFORMATION OF ELLIPTIC INTEGRALS, COMBINED WITH THE ALGORITHM OF THE ARITHMETICO-GEOMETRIC MEAN.

[Abstract.]

The algorithm of the arithmetico-geometric mean, so remarkable for its symmetry and convenience, was first used by Gauss many years before the brilliant era of Abel and Jacobi. The form which the theory of elliptic functions assumed under the hands of these eminent geometers, though extremely beautiful, might be improved from a practical point of view by a combination with the Gaussian algorithm. In the attempt to do this, the defects of the usual notation became very annoying, and gradually the new, simple, and consistent system of notations, as used in the following, resulted:

I assume for the type of an integral of the first species,

$$u = \int_0^{\varphi} \frac{a\,d\varphi}{\sqrt{a^2 - c^2 \sin^2\varphi}} = \int_0^{\varphi} \frac{a\,d\varphi}{\sqrt{a^2 \cos^2\varphi + b^2 \sin^2\varphi}}$$

$$= \int_0^{\varphi} \frac{d\varphi}{\sqrt{1 - \gamma^2 \sin^2\varphi}} = \int_0^{\varphi} \frac{d\varphi}{\sqrt{\cos^2\varphi + \beta^2 \sin^2\varphi}} = \varphi\gamma \qquad (1)$$

For the inverse of this I write $u_{-\gamma} = \varphi$. (2)

By (1) we have the modulus $\gamma = \frac{c}{a}$ and the complementary modulus $\beta = \frac{b}{a}$. The letters γ and β are used throughout as symbols for $\frac{c}{a}$ and $\frac{b}{a}$, respectively, and are expressed in a, b, and c whenever required.

In the theory of elliptic functions, sin am$u$, cos am$u$, Δ am$u$ (Jacobi's notation) or sn$u$, cn$u$, dn$u$ (Gudermann's notation), the elliptic quadrant K (Jacobi) is the numerical unit of their period. Consistency requires the use of the quadrant as a unit for trigonometric functions also. Let $\rfloor$ denote a circular quadrant (ordinarily denoted $\frac{\pi}{2}$); then we have, by the notation just explained,

$$\int_0^{\rfloor} \frac{d\varphi}{\sqrt{1-\gamma^2 \sin^2\varphi}} = \rfloor_\gamma \ (= K \text{ of Jacobi}). \tag{3}$$

The complementary integral then

$$\int_0^{\rfloor} \frac{d\varphi}{\sqrt{1-\beta^2 \sin^2\varphi}} = \rfloor_\beta \ (= K' \text{ of Jacobi}). \tag{4}$$

If n is an integer, then, and only then, $(n \rfloor)_\gamma = n \rfloor_\gamma$. (5)

Thus we should be careful in distinguishing between integrals such as

$$(\tfrac{1}{2}\rfloor)_\gamma = \int_0^{\frac{1}{2}\rfloor} \frac{d\varphi}{\sqrt{1-\gamma^2 \sin^2\varphi}} \text{ and } \tfrac{1}{2}\rfloor_\gamma = \tfrac{1}{2}\int_0^{\rfloor} \frac{d\varphi}{\sqrt{1-\gamma^2 \sin^2\varphi}}$$

According to the system of notation just explained, it is unnecessary to use the Jacobian am or the Gudermannian n, neither of which define the functional relation completely, and we write simply

$$\begin{aligned}
\sin\varphi &= \sin u_{-\gamma} \ (= \sin \mathrm{am}u \text{ of Jacobi or } \mathrm{sn}u \text{ of Gudermann}) \\
\cos\varphi &= \cos u_{-\gamma} \ (= \cos \mathrm{am}u \text{ of Jacobi or } \mathrm{cn}u \text{ of Gudermann}) \\
\sqrt{1-\gamma^2\sin^2\varphi} = \Delta\varphi &= \Delta u_{-\gamma} \ (= \Delta\, \mathrm{am}u \text{ of Jacobi or } \mathrm{dn}u \text{ of Gudermann})
\end{aligned} \tag{6}$$

I remark that none of the usual notations indicate the modulus, and a grave objection to Gudermann's is that it is apt to give the impression that sn$u$ and cn$u$ are not an ordinary sine and cosine. I shall now give in this notation a number of well-known relations, of which use will be made hereafter. The theorem of addition is, if u and v are two integrals to the modulus γ,

$$\sin(u \pm v)_{-\gamma} = \sin u_{-\gamma} \cos v_{-\gamma} \Delta v_{-\gamma} \pm \sin v_{-\gamma} \cos u_{-\gamma} \Delta u_{-\gamma}$$
$$\div 1 - \gamma^2 \sin^2 u_{-\gamma} \sin v_{-\gamma}$$

$$\cos(u \pm v)_{-\gamma} = \cos u_{-\gamma} \cos v_{-\gamma} \mp \sin u_{-\gamma} \Delta u_{-\gamma} \sin v_{-\gamma} \Delta v_{-\gamma}$$
$$\div 1 - \gamma^2 \sin^2 u_{-\gamma} \sin v_{-\gamma}$$

$$\Delta(u \pm v)_{-\gamma} = \Delta u_{-\gamma} \Delta v_{-\gamma} \mp \gamma^2 \sin u_{-\gamma} \cos u_{-\gamma} \sin v_{-\gamma} \cos v_{-\gamma}$$
$$\div 1 - \gamma^2 \sin^2 u_{-\gamma} \sin v_{-\gamma} \qquad (7)$$

We have
$$\sin(\pm \lrcorner) = \pm 1$$
$$\cos(\pm \lrcorner) = 0$$
$$\Delta(\pm \lrcorner) = \beta \qquad (8)$$

therefore, replacing v by $\lrcorner\gamma$, we have

$$\sin(u \pm \lrcorner\gamma) = \pm \frac{\cos u_{-\gamma}}{\Delta u_{-\gamma}}$$
$$\cos(u \pm \lrcorner\gamma) = \pm \beta \frac{\sin u_{-\gamma}}{\Delta u_{-\gamma}}$$
$$\Delta(u \pm \lrcorner\gamma) = \frac{\beta}{\Delta u_{-\gamma}} \qquad (9)$$

Replacing in these u by $u \pm \lrcorner\gamma$, we have

$$\sin(u \pm 2\lrcorner\gamma) = -\sin u_{-\gamma}$$
$$\cos(u \pm 2\lrcorner\gamma) = -\cos u_{-\gamma}$$
$$\Delta(u \pm 2\lrcorner\gamma) = \Delta u_{-\gamma} \qquad (10)$$

It follows, replacing in these u by $u + 2\lrcorner\gamma$, that $4\lrcorner\gamma$ is the complete period of the elliptic sine and cosine and $2\lrcorner\gamma$ that of the delta.

Placing $u = v$, we have the duplication formulæ:

$$\sin(2u)_{-\gamma} = 2 \sin u_{-\gamma} \cos u_{-\gamma} \Delta u_{-\gamma} \div 1 - \gamma^2 \sin^4 u_{-\gamma}$$
$$\cos(2u)_{-\gamma} = \cos^2 u_{-\gamma} - \sin^2 u_{-\gamma} \Delta^2 u_{-\gamma} \div 1 - \gamma^2 \sin^4 u_{-\gamma}$$
$$\Delta(2u)_{-\gamma} = \Delta^2 u_{-\gamma} - \gamma^2 \sin^2 u_{-\gamma} \cos^2 u_{-\gamma} \div 1 - \gamma^2 \sin^4 u_{-\gamma} \qquad (11)$$

Replacing in these u by $\frac{1}{2} u$ and solving, we have the dimidiation formulæ:

$$\sin^2\left(\frac{u}{2}\right)_{-\gamma} = 1 - \cos u_{-\gamma} \div 1 + \Delta u_{-\gamma}$$
$$\cos^2\left(\frac{u}{2}\right)_{-\gamma} = \Delta u_{-\gamma} + \cos u_{-\gamma} \div 1 + \Delta u_{-\gamma}$$
$$\Delta^2\left(\frac{u}{2}\right)_{-\gamma} = \beta^2 + \Delta u_{-\gamma} + \gamma^2 \cos u_{-\gamma} \div 1 + \Delta u_{-\gamma} \qquad (12)$$

Jacobi's imaginary transformation consists in assuming

$$\sin\varphi = i\tan\psi$$

or

$$\cos\varphi = \frac{1}{\cos\psi}$$

or

$$\Delta\,\varphi = \frac{1}{\cos\psi}\sqrt{1-\beta^2\sin^2\psi} = \frac{1}{\cos\varphi}\,\Delta\,(\psi\beta)_{-\gamma} \qquad (13)$$

then

$$u = \int_0^{\varphi} \frac{d\varphi}{\Delta\varphi} = i\int_0^{\psi} \frac{d\psi}{\sqrt{1-\beta^2\sin^2\psi}}$$

or

$$u = \varphi\gamma = i\,\psi\beta \qquad (14)$$

therefore, by (13),

$$\sin u_{-\gamma} = i\tan\left(\frac{u}{i}\right)_{-\beta} = \frac{1}{i}\tan\,(ui)_{-\beta}$$

$$\cos u_{-\gamma} = \frac{1}{\cos\,\left(\frac{u}{i}\right)_{-\beta}} = \frac{1}{\cos\,(ui)_{-\beta}}$$

$$\Delta\,u_{-\gamma} = \frac{1}{\cos\,\left(\frac{u}{i}\right)_{-\beta}}\,\Delta\left(\frac{u}{i}\right)_{-\beta} = \frac{1}{\cos\,(ui)_{-\beta}}\,\Delta\,(ui)_{-\beta} \qquad (15)$$

Using these relations in (7), we obtain the following formulæ for elliptic functions, with complex arguments and complementary moduli:

$$\sin\,(u \pm vi)_{-\gamma} = \sin u_{-\gamma}\ \ \Delta\,v_{-\beta} \pm i\cos u_{-\gamma}\ \ \Delta\,u_{-\gamma}\sin v_{-\beta}\cos v_{-\beta}$$
$$\div\, 1 - \Delta^2 u_{-\gamma}\sin^2 v_{-\beta}$$

$$\cos\,(u \pm vi)_{-\gamma} = \cos u_{-\gamma}\cos v_{-\beta} \mp i\sin u_{-\gamma}\ \ \Delta\,u_{-\gamma}\sin v_{-\beta}\ \ \Delta\,v_{-\beta}$$
$$\div\, 1 - \Delta^2 u_{-\gamma}\sin^2 v_{-\beta}$$

$$\Delta\,(u \pm vi)_{-\gamma} = \Delta\,u_{-\gamma}\cos v_{-\beta}\,\Delta\,v_{-\beta} \mp \gamma^2\,i\sin u_{-\gamma}\cos u_{-\gamma}\sin v_{-\beta}$$
$$\div\, 1 - \Delta^2 u_{-\gamma}\sin^2 v_{-\beta} \qquad (16)$$

We have

$$\sin\,(\rfloor\beta)_{-\beta} = 1$$

$$\cos\,(\rfloor\beta)_{-\beta} = 0$$

$$\Delta\,(\rfloor\beta)_{-\beta} = \gamma \qquad (17)$$

therefore, replacing in (16) v by $\rfloor\beta$, we have

$$\sin\,(u \pm \rfloor\beta\, i)_{-\beta} = \frac{1}{\gamma \sin u_{-\gamma}}$$

$$\cos\,(u \pm \rfloor\beta\, i)_{-\beta} = \mp\, i\, \frac{\Delta\, u_{-\gamma}}{\gamma \sin u_{-\gamma}}$$

$$\Delta\,(u \pm \rfloor\beta\, i)_{-\beta} = \mp\, i \cot u_{-\gamma} \tag{18}$$

Placing in these $u \pm \rfloor\beta\, i$ for u, we have

$$\sin\,(u \pm 2\rfloor\beta\, i)_{-\gamma} = \sin u_{-\gamma}$$

$$\cos\,(u \pm 2\rfloor\beta\, i)_{-\gamma} = -\cos u_{-\gamma}$$

$$\Delta\,(u \pm 2\rfloor\beta\, i)_{-\gamma} = -\,\Delta\, u_{-\gamma} \tag{19}$$

It follows, replacing in these u by $u \pm 2\rfloor\beta\, i$, that $4\rfloor\beta\, i$ is the imaginary period of the elliptic cosine and delta and $2\rfloor\beta\, i$ that of the sine. We have then, if m and μ are integers,

$$\sin\,(u + 4\,m\rfloor\gamma + 2\,\mu\rfloor\beta\, i)_{-\gamma} = \sin u_{-\gamma}$$

$$\cos\,(u + 4\,m\rfloor\gamma + 4\,\mu\rfloor\beta\, i)_{-\gamma} = \cos u_{-\gamma}$$

$$\Delta\,(u + 2\,m\rfloor\gamma + 4\,\mu\rfloor\beta\, i)_{-\gamma} = \Delta\, u_{-\gamma} \tag{20}$$

The general problem of transformation may be stated thus: Assuming

$$\int_0^{\varphi} \frac{d\varphi}{\sqrt{a^2 - c^2 \sin^2\varphi}} = \int_0^{\varphi'} \frac{d\varphi'}{\sqrt{a'^2 - c'^2 \sin^2\varphi'}} \text{ or } \frac{1}{a}\,\varphi_\gamma = \frac{1}{a'}\,\varphi'_{\gamma'} \tag{21}$$

then it is required to discover the relations between the given quantities φ, a, γ and φ', a', γ'.

Before treating of the special subject of this paper (the quadric transformation), a short exposition of some important points of the general problem of transformation, slightly modified from Abel (see Enneper's Elliptische Functionen, page 239–246), will be given.

We have, by (21),

$$\sin \varphi = \sin\left(\frac{a}{a'}\,\varphi'_{\gamma'}\right)_{-\gamma} = f(\sin\varphi') = f\left\{\sin\left(\frac{a'}{a}\,\varphi_\gamma\right)_{-\gamma'}\right\} \tag{22}$$

where f denotes the unknown relation between $\sin\varphi$ and $\sin\varphi'$.

But we have, by (20),

$$\sin\,(\varphi'\gamma' + 4\,m' \rfloor\gamma' + 2\,\mu' \rfloor\beta'\, i)_{-\gamma'}$$

$$= \sin\left\{\frac{a'}{a}\,(\varphi\gamma + 4\,m \rfloor\gamma + 2\,\mu \rfloor\beta\, i)\right\}_{-\gamma'} \tag{23}$$

therefore,
$$m' \rfloor\gamma' = \frac{a'}{a}\, m \rfloor\gamma \tag{24}$$

$$\mu' \rfloor\beta' = \frac{a'}{a}\, \mu \rfloor\beta \tag{25}$$

$$\therefore \frac{a'}{a} = \frac{m'}{m}\cdot\frac{\rfloor\gamma'}{\rfloor\gamma} = \frac{\mu'}{\mu}\cdot\frac{\rfloor\beta'}{\rfloor\beta}$$

and
$$\frac{m'}{\mu'}\cdot\frac{\rfloor\gamma'}{\rfloor\beta'} = \frac{m}{\mu}\cdot\frac{\rfloor\gamma}{\rfloor\beta}$$

Anticipating here the definition of the highly important constant, the nome q, which is such a prominent feature in the brilliant researches of Jacobi and Abel, we have

$$q = e^{-2\frac{\rfloor\beta}{\rfloor\gamma}\rfloor} \tag{26}$$

and the nome q' of the transformed integral is

$$q' = e^{-2\frac{\rfloor\beta'}{\rfloor\gamma'}\rfloor} = e^{-2\frac{m'\,\mu\,\rfloor\beta}{\mu'\,m\,\rfloor\gamma}\rfloor} = q^{\frac{m'\,\mu}{\mu'\,m}} \tag{27}$$

Thus it appears that the nomes of the given and transformed integrals are in a relation

$$q^n = q'^{\,n'}$$

where n and n' are integers, and, if $n = 1$ and $n' = 2$, we have the quadric transformation.

Landen's transformation consists in assuming

$$\sin\,(2\varphi' - \varphi) = \frac{c}{a}\sin\varphi \tag{28}$$

which is Legendre's convenient form for computing the amplitude φ'. Differentiating, we have

$$(2d\varphi' - d\varphi)\cos\,(2\varphi' - \varphi) = \frac{c}{a}\cos\varphi\; d\varphi$$

or
$$\frac{d\varphi}{a\,\Delta\,\varphi} = \frac{d\varphi'}{\frac{1}{2}\,(a\,\Delta\,\varphi + c\cos\varphi)} \tag{29}$$

Solving for φ, we have

$$\tan\varphi = \frac{a\sin 2\varphi'}{c + a\cos 2\varphi'} = \frac{a\tan\varphi'}{a' - b'\tan^2\varphi'} \tag{30}$$

$$\sin\varphi = \frac{2a\sin\varphi'\cos\varphi'}{\sqrt{a^2 + 2ac\cos 2\varphi' + c^2}} = \frac{a\sin\varphi'\cos\varphi'}{a'\Delta\varphi'} \tag{31}$$

$$\cos\varphi = \frac{c + a\cos 2\varphi'}{2a'\Delta\varphi'} = \frac{1}{c}\left(a'\Delta\varphi' - \frac{b'}{\Delta\varphi'}\right) \tag{32}$$

$$\Delta\varphi = \frac{1}{a}\left(a'\Delta\varphi' + \frac{b'}{\Delta\varphi'}\right) \tag{33}$$

where we have placed

$$\tfrac{1}{2}(a + c) = a'\,;\ \tfrac{1}{2}(a - c) = b'\,;$$

$$\sqrt{ac} = c'\,;\ \sqrt{a'^2 - c'^2\sin^2\varphi'} = a'\Delta\varphi' \tag{34}$$

From (32) and (33) follows

$$a'\Delta\varphi' = \tfrac{1}{2}(a\Delta\varphi + c\cos\varphi) \tag{35}$$

$$\frac{b'}{\Delta\varphi'} = \tfrac{1}{2}(a\Delta\varphi - c\cos\varphi) \tag{36}$$

and (29) becomes $$\frac{d\varphi}{a\Delta\varphi} = \frac{d\varphi'}{a'\Delta\varphi'} \tag{37}$$

the integral is $$\frac{1}{a}\varphi\gamma = \frac{1}{a'}\varphi'\gamma' \tag{38}$$

The first and third formula of (34) give the first step in the algorithm of the arithmetico-geometric mean, and the first two follow from (35) and (36) by placing $\varphi = 0 = \varphi'$, *i. e.*, they are relations at the lower limit of the integrals, corresponding to (35) and (36).

Assuming $$\sin(2\varphi'' - \varphi') = \frac{c'}{a'}\sin\varphi' \tag{28'}$$

$$a'' = \tfrac{1}{2}(a' + c')\,;\ b'' = \tfrac{1}{2}(a' - c')\,;\ c'' = \sqrt{a'c'} \tag{34'}$$

then we have $$\frac{1}{a}\varphi\gamma = \frac{1}{a'}\varphi'\gamma' = \frac{1}{a''}\varphi''\gamma'' \tag{38'}$$

Proceeding in this manner the amplitudes will very rapidly reach a limit $\varphi^{(\infty)}$, while simultaneously a and c tend to become equal to their common limit, the arithmetico-geometric mean of a and c. Gauss, when investigating its functional properties, denotes

this by $M(a, c)$; elsewhere he uses the notation $a^{(\infty)}$ or $c^{(\infty)}$, which is sufficiently distinct for our purpose.

At the limit we have $a^{(\infty)} \Delta\, \varphi^{(\infty)} = c^{(\infty)} \cos \varphi^{(\infty)}$, therefore,

$$\frac{1}{a}\, \varphi\gamma = \frac{1}{a'}\, \varphi'\gamma' = \frac{1}{a''}\, \varphi''\gamma'' = \;.\;.\;.\;.\;.\; \frac{1}{a^{(\infty)}}\, \varphi^{(\infty)}$$

$$\left(= \int_0^{\varphi^{(\infty)}} \frac{d\varphi^{(\infty)}}{c^{(\infty)} \cos \varphi^{(\infty)}} = \frac{1}{c^{(\infty)}} \tan \tfrac{1}{2} \left(\lrcorner + \varphi^{(\infty)}\right)\right) \qquad (38^{(\infty)})$$

Let $\varphi = \lrcorner$ then $\varphi' = \lrcorner'$; $\varphi'' = \lrcorner'' \;.\;.\;.\;.\; \varphi^{(\infty)} = \lrcorner^{(\infty)}$ and

$$\frac{1}{a} \lrcorner\gamma = \frac{1}{a'} \lrcorner'\gamma' = \frac{1}{a''} \lrcorner''\gamma'' = \;.\;.\;.\;.\;.\; \frac{1}{a^{(\infty)}} \lrcorner_1^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}} \tan \tfrac{1}{2} \left(\lrcorner + \lrcorner^{(\infty)}\right)\right) \qquad (39^{(\infty)})$$

This transformation can be applied also to the more general form:

$$I = \int_0^{\varphi} \frac{d\varphi}{a\, \Delta\, \varphi} f(\sin \varphi, \cos \varphi, \Delta\, \varphi) \qquad (40)$$

for if, simultaneously to the above algorithm, we express $\sin \varphi$, $\cos \varphi$, $\Delta\, \varphi$ in terms of $\sin \varphi'$, $\cos \varphi'$, $\Delta\, \varphi'$, and these again in terms of $\sin \varphi''$, $\cos \varphi''$, $\Delta\, \varphi''$, etc., by means of (31), (32), (33), we arrive, after a few transformations, at the form

$$I = \int_0^{\varphi^{(\infty)}} \frac{d\varphi^{(\infty)}}{c^{(\infty)} \cos \varphi^{(\infty)}} f^{(\infty)} \left(\sin \varphi^{(\infty)}, \cos \varphi^{(\infty)}\right) \qquad (41)$$

which is an elementary form if $f(\sin \varphi, \cos \varphi, \Delta\, \varphi)$ is rational with respect to $\sin \varphi$, $\cos \varphi$, $\Delta\, \varphi$.

In tracing this process backwards, the quantities may be distinguished at the several steps by subprimes, so that we have, at the first backward step,

$$\sin (2\varphi - \varphi_{,}) = \frac{c_{,}}{a_{,}} \sin \varphi_{,} = \frac{a - b}{a + b} \sin \varphi_{,} \qquad (28_{,})$$

$$a = \tfrac{1}{2}(a_{,} + c_{,})\,;\; b = \tfrac{1}{2}(a_{,} - c_{,})\,;\; c = \sqrt{a_{,} c_{,}} \qquad (34_{,})$$

Adding, and then also subtracting, $\sin \varphi_{,}$ from $(28_{,})$ and dividing the difference by the sum, we have the following convenient formula, also given by Legendre:

$$\tan (\varphi_{,} - \varphi) = \frac{b}{a} \tan \varphi \qquad (42)$$

Solving ($34_{,}$) for $a_{,}$, $b_{,}$, $c_{,}$, we have

$$a_{,} = a + b;\ b_{,} = 2\sqrt{ab};\ c_{,} = a - b$$

In order to have again the convenient algorithm of the arithmetico-geometric mean, it is preferable to assume.

$$a_1 = \tfrac{1}{2}\, a_{,} = \tfrac{1}{2}\,(a + b);\, b_1 = \tfrac{1}{2}\, b_{,} = \sqrt{ab};\, c_1 = \tfrac{1}{2}\, c_{,} = \tfrac{1}{2}\,(a - b) \quad (43)$$

For the second step assume

$$\tan(\varphi_{,,} - \varphi_{,}) = \frac{b_1}{a_1} \tan \varphi_{,} \qquad (42_1)$$

$$a_2 = \tfrac{1}{2}\,(a_1 + b_1);\ b_2 = \sqrt{a_1 b_1};\ c_2 = \tfrac{1}{2}\,(a_1 - b_1) \qquad (43_1)$$

We have then $\quad \dfrac{1}{a}\,\varphi\gamma = \dfrac{1}{2a_1}\,(\varphi_{,})\gamma_1 = \dfrac{1}{2^2 a_2}\,(\varphi_{,,})\gamma_2 \qquad (44_1)$

Continuing this process, which diminishes the modulus, and is therefore called descending the scale of moduli, while the above is called ascending, the a and b will rapidly approach their arithmetico-geometric mean, $a_\infty = b_\infty$, while $\dfrac{1}{2^n}\,\varphi_{(n)}$ tends towards a limit which I shall denote ψ_∞. The limiting form of the integral is

$$\int_0^{\varphi(\infty)} \frac{d\varphi_{(\infty)}}{2^\infty\, b_\infty} = \frac{\psi_\infty}{b_\infty}$$

and we have

$$\frac{1}{a}\,\varphi\gamma = \frac{1}{2a_1}(\varphi_{,})\gamma_1 = \frac{1}{2^2 a_2}(\varphi_{,,})\gamma_2 = \ldots\ldots \frac{1}{2^\infty\, a_\infty}\,(\varphi_\infty)_0 \left(= \frac{\psi_\infty}{b_\infty} \quad (44^{(\infty)})\right.$$

If $\varphi = \rfloor$ then $\tfrac{1}{2}\,\varphi_{,} = \dfrac{1}{2^2}\,\varphi_{,,} = \ldots \dfrac{1}{2^n}\,\varphi_{(n)} = \ldots = \rfloor$

and we have

$$\frac{1}{a}\,\rfloor\gamma = \frac{1}{a_1}\,\rfloor\gamma_1 = \frac{1}{a_2}\,\rfloor\gamma_2 = \ldots\ldots\ldots \frac{1}{a_\infty}\,\rfloor_0 \left(= \frac{1}{b_\infty}\,\rfloor \quad (45_{(\infty)})\right.$$

This remarkable value for the complete integral was discovered by Gauss by means of a different transformation, known as Gauss'. This may be deduced as follows: Assume in place of ($44^{(\infty)}$) the following series of relations

$$\frac{1}{a}\,\varphi\gamma = \frac{1}{a_1}\,(\psi_1)\gamma_1 = \frac{1}{a_2}\,(\psi_2)\gamma_2 = \ldots\ldots \frac{1}{a_\infty}\,(\psi_\infty)_0 \left(= \frac{1}{b_\infty}\,\psi_\infty \quad (44_\infty)\right.$$

To discover the relations for the first step we have to determine ψ_1 from the equations

$$(\psi_1)\gamma_1 = \tfrac{1}{2}(\varphi_{,})\gamma_1 = \frac{a_1}{a}\varphi\gamma \qquad (46)$$

Place in (12) $u = (\varphi_{,})\gamma_1$, then $\frac{1}{2}u = (\psi_1)\gamma_1$ and $u_{-\gamma_1} = \varphi_{,}; \left(\frac{u}{2}\right)_{-\gamma_1} = \psi_1$, and consequently

$$\begin{aligned}
\sin^2\psi_1 &= 1 - \cos\varphi_{,} & &\div 1 + \Delta\varphi_{,}\\
\cos^2\psi_1 &= \Delta\varphi_{,} + \cos\varphi_{,} & &\div 1 + \Delta\varphi_{,}\\
\Delta^2\psi_1 &= \beta_1^2 + \Delta\varphi_{,} + \gamma_1^2\cos\varphi_{,} & &\div 1 + \Delta\varphi_{,}
\end{aligned} \qquad (47)$$

From (32) and (33) we derive with due regard to (43)

$$\begin{aligned}
a_1\,\Delta\varphi_{,} &= \tfrac{1}{2}\left(a\,\Delta\varphi + \frac{b}{\Delta\varphi}\right)\\
c_1\,\Delta\varphi_{,} &= \tfrac{1}{2}\left(a\,\Delta\varphi - \frac{b}{\Delta\varphi}\right)
\end{aligned} \qquad (48)$$

and eliminating $\varphi_{,}$ from (47) by means of (48) there result the relations

$$\begin{aligned}
\sin^2\psi_1 &= \frac{a_1}{c_1}\cdot\frac{1-\Delta\varphi}{1+\Delta\varphi}\\
\cos^2\psi_1 &= \frac{a\,\Delta\varphi - b}{c_1(1+\Delta\varphi)}\\
\Delta^2\psi_1 &= \frac{a\,\Delta\varphi + b}{a_1(1+\Delta\varphi)}
\end{aligned} \qquad (49)$$

whence also

$$\begin{aligned}
\sin\varphi &= \frac{a\sin\psi_1}{a_1 + c_1\sin^2\psi_1}\\
\cos\varphi &= \frac{a_1\cos\psi_1\,\Delta\psi_1}{a_1 + c_1\sin^2\psi_1}\\
\Delta\varphi &= \frac{a_1 - c_1\sin^2\psi_1}{a_1 + c_1\sin^2\psi_1}
\end{aligned} \qquad (50)$$

This is Gauss' transformation. For practical use it is far less convenient than that given above.

Instead of (46) we might have assumed

$$m(\theta_1)\gamma_1 = n(\varphi_{,})\gamma_1 = 2n\frac{a_1}{a}\varphi\gamma \quad (m \text{ and } n \text{ integers}) \qquad (51)$$

For any special values of m and n we can express, by means of

the addition theorem, the elliptic functions of $\{m\,(\theta_1)\gamma_1\}-\gamma_1$ in terms of those of θ_1, and in the same manner those of $\{n\,(\varphi_{,})\gamma_1\}-\gamma_1$ in terms of those of $\varphi_{,}$. Since we know φ in terms of $\varphi_{,}$, we can eliminate $\varphi_{,}$ and obtain a relation between θ_1 and φ, which would be a new transformation. However, we need not expect to discover in this manner any substitution sufficiently simple for practical use.

The substitutions given above may of course be applied also to the complementary integral, and, since interesting relations will be thus discovered, I place the different series of forms together for comparison.

$$\frac{1}{a}\,\varphi\gamma = \frac{1}{a'}\,\varphi'\gamma' = \frac{1}{a''}\,\varphi''\gamma'' = \ldots\ldots\ldots\ldots = \frac{1}{a^{(\infty)}}\,\varphi_1{}^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}}\, l \tan \tfrac{1}{2}\,(\rfloor + \varphi^{(\infty)})\right)$$

$$= \frac{1}{2a_1}\,(\varphi_{,})\gamma_1 = \frac{1}{2^2 a_2}\,(\varphi_{,,})\gamma_2 = \ldots\ldots\ldots = \frac{1}{a_\infty} \lim \frac{\varphi_{(n)}}{2^n}$$

$$\left(= \frac{1}{b_\infty}\,\psi_\infty\right.$$

$$= \frac{1}{a_1}\,(\psi_1)\gamma_1 = \frac{1}{a_2}\,(\psi_2)\gamma_2 = \ldots\ldots\ldots = \frac{1}{a_\infty}\,(\psi_\infty)_0$$

$$\left(= \frac{1}{b_\infty}\,\psi_\infty \right. \qquad (52\gamma)$$

$$\frac{1}{a}\,\varphi\beta = \frac{1}{a_1}\,(\varphi_1)\beta_1 = \frac{1}{a_2}\,(\varphi_2)\beta_2 = \ldots\ldots\ldots = \frac{1}{a_\infty}\,(\varphi_\infty)_1$$

$$\left(= \frac{1}{b_\infty}\, l \tan \tfrac{1}{2}\,(\rfloor + \varphi_\infty)\right)$$

$$= \frac{1}{2a_1}\,(\varphi_{-1})\beta' = \frac{1}{2^2 a''}\,(\varphi_{-2})\beta'' = \ldots\ldots\ldots = \frac{1}{a^{(\infty)}} \lim \frac{\varphi_{-n}}{2^n}$$

$$\left(= \frac{1}{c^{(\infty)}}\,\psi^{(\infty)}\right.$$

$$= \frac{1}{a'}\,\psi'\beta' = \frac{1}{a''}\,\psi''\beta'' = \ldots\ldots\ldots\ldots = \frac{1}{a^{(\infty)}}\,\psi_0{}^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}}\,\psi^{(\infty)}\right. \qquad (52\beta)$$

$$\frac{1}{a}\,\rfloor\gamma = \frac{1}{a'}\,\rfloor'\gamma' = \frac{1}{a''}\,\rfloor''\gamma'' = \ldots\ldots\ldots\ldots = \frac{1}{c^{(\infty)}}\,\rfloor_1{}^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}} \, l \tan \tfrac{1}{2} (\lrcorner + \lrcorner^{(\infty)})\right.$$

$$= \frac{1}{a_1} \lrcorner\gamma_1 = \frac{1}{a_2} \lrcorner\gamma_2 = \dots\dots\dots\dots = \frac{1}{b_\infty} \lrcorner \quad (53\gamma)$$

$$\frac{1}{a} \lrcorner\beta = \frac{1}{a_1} (\lrcorner_1)\beta_1 = \frac{1}{a_2} (\lrcorner_2)\beta_2 = \dots\dots\dots = \frac{1}{b_\infty} (\lrcorner^{(\infty)})_1$$

$$\left(= \frac{1}{b_\infty} \, l \tan \tfrac{1}{2} (\lrcorner + \lrcorner_\infty)\right.$$

$$= \frac{1}{a'} \lrcorner\beta' = \frac{1}{a''} \lrcorner\beta'' = \dots\dots\dots\dots = \frac{1}{c^{(\infty)}} \lrcorner \quad (53\beta)$$

We easily deduce the symmetrical relations

$$\varphi_1^{(\infty)} (\varphi_\infty)_1 = \psi_\infty \psi^{(\infty)} \quad (54)$$

$$\lrcorner_1^{(\infty)} (\lrcorner_\infty)_1 = \lrcorner^2 \quad (55)$$

This last equation is well known; it appears here, however, as a particular case of a more general relation. The quantity ψ_∞ is the argument of the θ functions and then usually denoted x; $\varphi_1^{(\infty)}$ is then denoted by x'; Schellbach has $\frac{\nu}{2}$ for $(\lrcorner_\infty)_1$ and $\frac{\nu'}{2}$ for $\lrcorner_1^{(\infty)}$, while Hoüel, in his Recueil de Tables, has ρ and ρ', respectively.

Other relations are

$$\frac{\varphi_1^{(\infty)}}{\lrcorner_1^{(\infty)}} = \frac{\psi_\infty}{\lrcorner}; \quad \frac{\varphi_1^{(\infty)}}{\lrcorner} = \frac{\psi_\infty}{(\lrcorner_\infty)_1}; \quad \frac{\varphi_1^{(\infty)2}}{\lrcorner_1^{(\infty)}} = \frac{\varphi_1^{(\infty)} \psi_\infty}{\lrcorner} = \frac{\psi_\infty^{\,2}}{(\lrcorner_\infty)_1} \quad (56)$$

$$\frac{(\varphi_\infty)_1}{(\lrcorner_\infty)_1} = \frac{\psi^{(\infty)}}{\lrcorner}; \quad \frac{(\varphi_\infty)_1}{\lrcorner} = \frac{\psi^{(\infty)}}{\lrcorner_1^{(\infty)}}; \quad \frac{(\varphi_\infty)_1^{\,2}}{(\lrcorner_\infty)_1} = \frac{(\varphi_\infty)_1 \psi^{(\infty)}}{\lrcorner} = \frac{\psi^{(\infty)2}}{\lrcorner_1^{(\infty)}} \quad (57)$$

The following expressions for the nome q can now be given:

$$q = e^{-2 \frac{\lrcorner\beta}{\lrcorner\gamma} \lrcorner} = e^{-2 (\lrcorner_\infty)_1}$$

$$= e^{-2 \frac{b_\infty}{c^{(\infty)}} \lrcorner} = e^{-2 \frac{b_\infty}{a} \lrcorner\beta} \quad (58)$$

The first form is simply Jacobi's definition; the second gives, since

$$(\lrcorner_\infty)_1 = l \tan \tfrac{1}{2} (\lrcorner + \lrcorner_\infty) \quad (59)$$

$$q = \cot^2 \tfrac{1}{2} (\lrcorner + \lrcorner_\infty) \quad (60)$$

This is one of the best formulæ for computing q, especially if the modulus does not differ much from unity. The third form may be

used if b and c are not very different, for in that case the algorithm of the arithmetico-geometric mean converges equally fast in both directions. If either b or c is very near to a, the process may converge in one direction so slowly that the formula becomes nearly inapplicable.

The fourth form may be transformed to a new formula, which is more convenient than any given. In (52β) place $\varphi = 2^n \lrcorner$, then, since

$$\varphi_1 = 2^{n-1} \lrcorner\,; \ \varphi_1 = 2^{n-2} \lrcorner\,; \ \varphi_3 = 2^{n-3} \lrcorner \ \ldots .$$
$$\varphi_n = \lrcorner\,; \ \varphi_{n+1} = (2^n \lrcorner)_{n+1} \ \ldots .$$

we have

$$\frac{2^n}{a} \lrcorner \beta = \frac{2^{n-1}}{a_2} \lrcorner \beta_1 = \frac{2^{n-2}}{a_2} \lrcorner \beta_2 = \ldots .$$
$$= \frac{1}{a_n} \lrcorner \beta_n = \frac{1}{a_{n+1}} \left\{ (2^n \lrcorner)_{n+1} \right\} \beta_{n+1} \qquad (61)$$

But we have by (28) $\qquad \sin\left(2(2_n \lrcorner)_{n+1} - \lrcorner\right) = \dfrac{b_n}{a_n} \sin \lrcorner$

or

$$2 \sin^2 (2^n \lrcorner)_{n+1} - 1 = \frac{b_n}{a_n}$$
$$\therefore \sin (2^n \lrcorner)_{n+1} = \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}$$

If we suppose $a_n = b_n = b_\infty$ within the precision of the computation, c_n will be very small, yet not zero. We have then

$$\frac{1}{a^\infty} \lrcorner \beta_n = \frac{1}{a_{n+1}} \left\{ (2^n \lrcorner)_{n+1} \right\} \beta_{n+1}$$
$$= \frac{1}{a_{n+1}}\, l \tan \tfrac{1}{2} \left(\lrcorner + (2^n \lrcorner)_{n+1} \right)$$
$$= \frac{1}{a_{n+1}}\, l \ \sqrt{\frac{1 + \sin (2_n \lrcorner)_{n+1}}{1 - \sin (2^n \lrcorner)_{n+1}}}$$
$$= \frac{1}{a_{n+1}}\, l \ \sqrt{\frac{1 + \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}}{1 + \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}}}$$

$$= \frac{1}{a_{n+1}} \; l \; \frac{1 + \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}}{\sqrt{\tfrac{1}{2}\left((1 - \frac{b_n}{a_n}\right)}}$$

$$= \frac{1}{b_\infty} \; l \; \frac{2\sqrt{a_n}}{\sqrt{\tfrac{1}{2}(a_n - b_n)}} \quad \text{(sufficiently near)}$$

$$= \frac{1}{b_\infty} \; l \; \frac{2^2\sqrt{a_n a_{n+1}}}{c_n} \quad \text{(sufficiently near)}$$

$$= \frac{1}{b_\infty} \; l \; \frac{2^2 a_n}{c_n} \quad \text{(sufficiently near)} \qquad (62)$$

therefore we have by (61)

$$\frac{1}{a} \rfloor \beta = \frac{2^{-n}}{b_\infty} \; l \; \frac{2^2 a_n}{c_n}$$

or $$\frac{b_\infty}{a} \rfloor \beta = l \left(\frac{2^2 a_n}{c_n}\right)^{2^{-n}}$$

$$l \left(2^2 a_n \cdot \frac{2^2 a_n}{c^2_{n-1}}\right)^{2^{-n}} = l \left(\frac{2^2 a_n}{c_{n-1}}\right)^{2^{-n+1}}$$

$$\text{since } c_n = \tfrac{1}{2}(a_{n-1} - b_{n-1}) = \frac{c^2_{n-1}}{2^2 a_n}$$

$$= l \left(2^2 a_n \cdot \frac{2^2 a_{n-1}}{c^2_{n-2}}\right)^{2^{-n+1}} = l \left(\frac{2^2 a_{n-1}}{c_{n-2}}\right)^{2^{-n+2}}$$

$$\text{if } a_{n-1} = \sqrt{a_n a_{n-1}}$$

$$= l \left(2^2 a_{n-1} \cdot \frac{2^2 a_{n-2}}{c^2_{n-3}}\right)^{2^{-n+2}} = l \left(\frac{2_n a_{n-2}}{c_{n-3}}\right)^{2^{-n+3}}$$

$$\text{if } a_{n-2} = \sqrt{a_{n-1} a_{n-2}}$$

. . . .

$$= l \left(2^2 a_3 \cdot \frac{2^2 a_2}{c_1^2}\right)^{2^{-2}} = l \left(\frac{2^2 a_2}{c_1}\right)^{2^{-1}}$$

$$\text{if } a_2 = \sqrt{a_3 a_2} \qquad (63)$$

$$= l \left(2^2 a_2 \cdot \frac{2^2 a_1}{c^2}\right)^{2^{-1}} = l \left(\frac{2^2 a_{\prime}}{c}\right)$$

$$\text{if } a_1 = \sqrt{a_2 a_1} \qquad (64)$$

Using (63) in the fourth form of (58) we have

$$q = \frac{c_1}{2^2\, a_2} = \frac{a - b}{2^3\, a_2} \tag{65}$$

and using (64) we have

$$q = \left(\frac{c}{2^2\, a_1}\right)^2 \tag{66}$$

The nome of the complementary integral is denoted by Jacobi and writers that follow him by q'. In our system this would be the notation for the nome of the integral $\varphi'\gamma'$; q'' that for $\varphi''\gamma''$, etc; also q_1 that of $(\varphi_1)\gamma_1$; q_2 of $(\varphi_2)\gamma_2$, etc. It is therefore better to follow the example of Broch, who denotes the nome of the complementary integral by p. We have then

$$p = e^{-2\frac{\rfloor\gamma}{\rfloor\beta}\rfloor} = e^{-2\rfloor_1^{(\infty)}} = e^{-2\frac{c^{(\infty)}}{b_\infty}\rfloor}$$

$$= \cot^2 \tfrac{1}{2}\left(\rfloor + \rfloor^{(\infty)}\right) = \frac{a - c}{2^3\, a''} = \left(\frac{b}{2^2\, a'}\right)^2 \tag{67}$$

where

$$\begin{aligned}
a^{(n-1)} &= \sqrt{a^{(n)}\, a^{(n-1)}} \\
a^{(n-2)} &= \sqrt{a^{n-1}\, a^{n-2}} \\
a^{(n-3)} &= \sqrt{a^{n-2}\, a^{n-3}} \\
&\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\
a'' &= \sqrt{a'''\, a''} \\
a' &= \sqrt{a''\, a'}
\end{aligned} \tag{68}$$

By (55) and the second forms of (58) and (67) we have the following relation between p and q

$$l p^{-\frac{1}{2}}\, l q^{-\frac{1}{2}} = \rfloor^2 \tag{69}$$

or in Briggian logarithms

$$\log\left\{\log p^{-\frac{1}{2}} \log q^{-\frac{1}{2}}\right\} = \log\left(\rfloor \log e\right)^2 = 9.6678084 \tag{69'}$$

or $$\log\left\{\log p^{-1}\ \log q^{-1}\right\} = 0.2698684 \tag{70}$$

By means of this relation we can always choose the shortest route to either p or q. It is easy to see that the nomes and com-

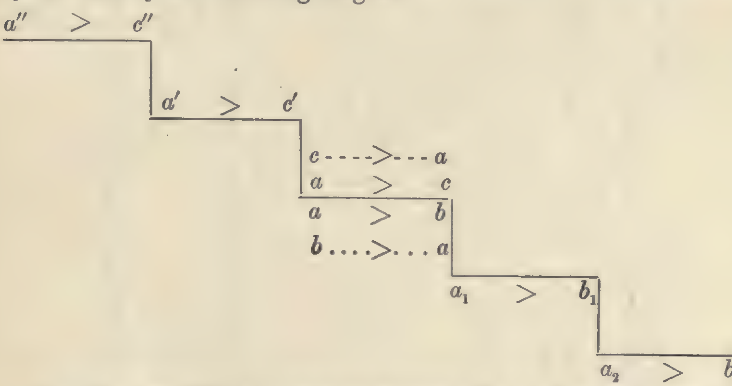
plementary nomes at the several steps of the modular scale are as follows

$$q_n = q^{2^{-n}} \dots q_2 = q^{2^{-2}}\,;\; q_1 = q^{2^{-1}}\; q = q;\; q' = q^2;\; q'' = q^{2^2} \dots\dots q^{(n)} = q^{2^n} \tag{71}$$

$$p_n = p^{2^n} \dots p_2 = p^{2^2}\,;\; p_1 = p^2\,;\; p = p;\; p' = p^2;\; p'' = p^{2^{-2}} \dots\dots p^{(n)} = p^{2^{-n}} \tag{72}$$

We have then in this transformation the simplest possible case of Abel's theorem (27); and because in ascending we pass to the square of the nome, it is called the quadric transformation.

The ascending transformation is possible in real quantities if $c > a$, for we have $M(c, a) = M(a, c)$. Also if $b > a$ we can use the descending transformation; and in either case we can, after one transformation, proceed in either direction. This may be symbolized by the following diagram



In order to exhibit the practical nature of the formulæ given, I shall make the necessary computations for the integral

$$u = \int_0^{\varphi} \frac{dy}{\sqrt{1 - \sin^2 75 \sin^2 \varphi}}$$

if $\varphi = 70^0$ and also for the complete integral.

Because $\gamma = \sin 75^0$ is $> \sqrt{\frac{1}{2}}$ we must use the ascending transformation. The computation for $70^0{}_{\sin 75^\circ}$ may be conveniently arranged as follows:

$a = 1$ $c = 0.9659258$	$\log c = 9.9849438$ $\log a = 0.0000000$	$\varphi = 70^\circ 00' 00.''00$ $2\varphi' - \varphi = 65\ 11\ 08.\ 84$	$\log \sin \varphi = 9.9729858$ $\log \sin (2\varphi' - \varphi) = 9.9579296$
$a' = 0.9829629$	$\log c' = 9.9924719$ $\log a' = 9.9925371$	$\varphi' = 67\ 35\ 34.\ 42$ $2\varphi'' - \varphi' = 67\ 34\ 19.\ 33$	$\log \sin \varphi' = 9.9659063$ $\log \sin (2\varphi'' - \varphi') = 9.9658411$
$\log c^{(\infty)} = \log c''$	$= 9.9925045$	$\varphi^{(\infty)} = \varphi'' = 67\ 34\ 56.\ 88$ $\frac{1}{2}(\lrcorner + \varphi^{(\infty)}) = 78\ 47\ 28.\ 44$	$\log \tan \frac{1}{2}(\lrcorner + \varphi^{(\infty)}) = 0.7029746$

$$
\begin{aligned}
\log \log \tan \tfrac{1}{2}(\lrcorner + \varphi^{(\infty)}) &= 9.8469397 \\
-\log \log e &= -\,9.6377843 \\
-\log c^{(\infty)} &= -\,9.9925045 \\
\hline
\log 70^{0}{}_{\sin 75^\circ} &= 0.2166509
\end{aligned}
$$

The computation of $\lrcorner_{\sin 75^\circ}$ by this process requires exactly the same amount of computation.

(By 53_γ) and (67), we have

$$\frac{1}{a}\lrcorner\gamma = \frac{1}{c^{(\infty)}}\lrcorner_1^{(\infty)} = \frac{1}{c^{(\infty)}}\, l\, p^{-\frac{1}{2}} = \frac{1}{c^{(\infty)}}\, l \sqrt{\frac{2^3 a''}{a-c}} = \frac{1}{c^{(\infty)}}\, l\, \frac{2^2 a'}{b} \qquad (73)$$

We compute then the complementary nome and from that the complete integral is found easily.

$$
\begin{array}{lrl}
 & -\log a'' = & -\ 9.9925045 \\
 & -\log 2^3 = & -\ 0.9030900 \\
a - c = 0.0340742 & \log (a - c) = & 8.5324257 \\
\hline
 & \log p \quad = & 7.6368312 \\
\text{Computation of } \lrcorner_{\sin 75^\circ} & \log p^{-\frac{1}{2}} = & 1.1815844 \\
\hline
 & \log \log p^{-\frac{1}{2}} = & 0.0724648 \\
 & -\log \log e \quad = & -\ 9.6377843 \\
 & -\log c^{(\infty)} = & -\ 9.9925045 \\
\hline
 & \log \lrcorner_{\sin 75^\circ} = & 0.4421760
\end{array}
$$

This method is far more simple than by amplitudes.

The computation of the same integral by the descending scale will now be shown, though it is longer in this case, and therefore less accurate.

$a = 1$ $b = 0.2588190$	$\log b = 9.4129962$ $\log a = 0.0000000$	$\varphi = 70^\circ 00' 00.''00$ $\varphi_{,} - \varphi = 35\ 24\ 59.\ 80$	$\log \tan \varphi = 0.4389341$ $\log \tan (\varphi_{,} - \varphi) = 9.8519303$
$a_1 = 0.6294095$ $b_1 = 0.5087425$	$\log b_1 = 9.7064981$ $\log a_1 = 9.7989333$	$\varphi_{,} = 105\ 24\ 59.\ 80$ $\varphi_{,,} - \varphi_{,} = 108\ 50\ 16.\ 04$	$\log \tan \varphi_{,} = 0.5594721_n$ $\log \tan (\varphi_{,,} - \varphi_{,}) = 0.4670369_n$
$a_2 = 0.5690760$ $b_2 = 0.5658687$	$\log b_2 = 9.7527157$ $\log a_2 = 9.7551704$	$\varphi_{,,} = 214\ 15\ 15.\ 84$ $\varphi_{,,,} - \varphi_{,,} = 214\ 06\ 14.\ 04$	$\log \tan \varphi_{,,} = 9.8331396$ $\log \tan (\varphi_{,,,} - \varphi_{,,}) = 9.8306850$
$a_3 = 0.5674724$	$\log b_3 = 9.7539430$ $\log a_3 = 9.7539447$	$\varphi_{,,,} = 428\ 21\ 29.\ 88$ $\varphi_{\text{iv}} - \varphi_{,,,} = 428\ 21\ 29.\ 60$	$\log \tan \varphi_{,,,} = 0.4014610$ $\log \tan (\varphi_{\text{iv}} - \varphi_{,,,}) = 0.4014593$
	$\log b_\infty = \log b_4 = 9.7539439$	$\varphi_{(\infty)} = \varphi_{\text{iv}} = 856\ 42\ 59.\ 48$ $\psi_\infty = 53\ 32\ 41.\ 22$ $= 192761.22$	

$$\begin{aligned}
\log \psi_\infty &= 5.2850197 \\
\log \text{arc } 1'' &= 4.6855749 \\
-\log b_\infty &= -9.7539439
\end{aligned}$$

Computation of $\lrcorner_{\sin 75^\circ}$

$$\begin{aligned}
\log 70^\circ_{\sin 75^\circ} &= -0.2166507 \\
\log \lrcorner &= 0.1961199 \\
-\log b_\infty &= -9.7539439 \\
\hline
\log \lrcorner_{\sin 75^\circ} &= 0.4421760
\end{aligned}$$

The computation of the nome q stands thus:

$$
\begin{array}{rlrl}
 & \log a_3 & = & 9.7539441 \\
\hline
 & -\log a_2 & = - & 9.7545572 \\
 & -\log 2^3 & = - & 0.9030900 \\
a - b = 0.7411810 & \log a - b & = & 9.8699243 \\
\hline
 & \log q & = & 9.2122771
\end{array}
$$

To check by (70), we have

$$
\begin{array}{ll}
\log p^{-1} = 2.3631688 & \log\log p^{-1} = 0.3734948 \\
\log q^{-1} = 0.7877229 & \log\log q^{-1} = 9.8963735 \\
\hline
 & \qquad\qquad\quad 0.2698683
\end{array}
$$

The above incomplete sketch is, I hope, sufficient to show the practical advantages from the introduction of the algorithm of the arithmetico-geometric mean into the theory of elliptic functions.

Mr. HALL spoke of the importance of the arithmetico-geometric mean in astronomy.

Mr. W. B. TAYLOR made a communication on

A CASE OF DISCONTINUITY IN ELLIPTIC ORBITS

around an empty center of gravitative force. Diminution of the minor axis of the attracted body's path (the major axis being constant) increases the ratio of distance at the two apses without limit, the "periapsis" continually approaching the attractive center, as long as the minor axis has a value, however small. But when this axis is made to vanish, and the motion is directly to the center of force, the body, instead of rebounding from it, as continuity would require, will pass through it, and describe an equal path on the opposite side, the orbit being at once doubled.

This paper was discussed by Messrs. BATES, CHRISTIE, HALL and others, and brought out a wide diversity of view as to the demeanor of a heavy point when coincident with an empty attracting center.

15TH MEETING. DECEMBER 3, 1884.

The Chairman presided.

Nineteen members and guests present.

Mr. M. H. DOOLITTLE made a communication on

THE VERIFICATION OF PREDICTIONS.

[Abstract.]

Mr. G. K. Gilbert has published (American Meteorological Journal, 8°, *Detroit;* September, 1884, pp. 166–172) a method of estimating the ratio of skill in predictions of occurrences and non-occurrences of a simple event. Adopting his notation, we have

$s =$ the sum or total number of cases,

$o =$ the number of occurrences,

$p =$ the number of predictions of occurrences,

$c =$ the number of coincidences or verifications,

$i =$ the inference-ratio, or that part of the success which is due to skill and not to chance, and which may be called the *degree of logical connection* between event and prediction.

Since success is proportional to each of the two fractions

$$\frac{c}{o} \text{ and } \frac{c}{p},$$

it may be represented by their product

$$\frac{c^2}{op}.$$

The fraction $\frac{o}{s}$ represents the ratio of random success, and therefore $\frac{op}{s}$ verifications out of p predictions are to be ascribed to chance and must be subtracted throughout. The remainders,

$$o - \frac{op}{s} \text{ and } p - \frac{op}{s},$$

represent fields which chance leaves for science to conquer; and

$$c - \frac{op}{s}$$

represents the portion of each which science does conquer. Hence

$$i = \frac{c - \frac{op}{s}}{o - \frac{op}{s}} \times \frac{c - \frac{op}{s}}{p - \frac{op}{s}} = \frac{(cs - op)^2}{op(s - o)(s - p)}.$$

By another method,

$\frac{c}{o}$ = the probability that any single occurrence will be predicted in some manner.

$\frac{p - c}{s - o}$ = the probability that any single date of non-occurrence will correspond to an unsuccessful prediction = the general probability of unskillful prediction in any case.

Subtract from the probability that any single occurrence will be predicted in some manner the general probability of unskillful prediction, and we have

$\frac{c}{o} - \frac{p - c}{s - o}$ = the probability that any given occurrence will be skillfully predicted.

In like manner

$\frac{c}{p}$ = the probability that any single prediction will be fulfilled in some manner.

$\frac{o-c}{s-p}$ = the general probability of unpredicted occurrence; which, in case of prediction, becomes probability of fortuitous fulfillment.

$\frac{c}{p} - \frac{o-c}{s-p}$ = the probability that any single prediction will be fulfilled by reason of a logical connection.

Since the skillful predictions are mingled indistinguishably with all the unskillful ones, and are vitiated accordingly, the value of the *vitiated* probability of the skillful prediction of any single occurrence may be represented by the product

$$i = \left(\frac{c}{o} - \frac{p-c}{s-o}\right) \times \left(\frac{c}{p} - \frac{o-c}{s-p}\right) = \frac{(cs-op)^2}{op(s-o)(s-p)},$$

as before.

Prof. C. S. Peirce (in *Science*, 1884, Nov. 14, Vol. IV, page 453) deduces the first of these factors as the unqualified value of i, making no allowance for the vitiation, and tacitly assuming that an assortment of predictions is the logical equivalent of a jumble of the same predictions. He obtains his result by the aid of the supposition that part of the predictions are made by an infallible prophet, and the others by a man ignorant of the future. If Prof. Peirce had called on omnipotence instead of omniscience, and supposed the predictions to have been obtained from a Djinn careful to fulfill a portion of them corresponding to the data, the remainder of the occurrences being produced by an unknown Djinn at random, he would have obtained by parallel reasoning the second factor, $\left(\frac{c}{p} - \frac{o-c}{s-p}\right)$. These Djinns represent, respectively, the known and unknown forces of nature, and gauge the prophet's knowledge with principal reference to the proportion of predictions fulfilled. Prof. Peirce's test refers principally to the proportion of occurrences predicted. His test eliminates sins of omission; the other, sins of commission; and both are necessary to a proper estimate of the prophet's comparative rectitude.

In the data cited by Mr. Gilbert from Finley's tornado predictions, $s = 2803$, $o = 51$, $p = 100$, and $c = 28$. By Mr. Gilbert's formula,

$$i = \frac{cs - op}{s(o + p - c) - op}$$

he obtains

$$i = .216.$$

Prof. Peirce obtains

$$i = .523.$$

I obtain

$$i = .142.$$

By making s, o, and i constant, and imposing conditions on p and c, we may obtain hypothetical data involving equal skill. Putting $c = p$, I infer that Mr. Finley would have manifested equal skill if he had made no false predictions of tornadoes, and, out of the 51, had predicted 7.35. Mr. Gilbert's formula gives 11.18, and Prof Peirce's 26.67. Putting $c = o$, I infer that he would also have manifested equal skill if he had included all the 51 tornadoes by making 323.7 predictions. Mr. Gilbert's formula gives 221.5, and Prof. Peirce's 1364.

Mr. Finley's entire success in predicting tornadoes is

$$\frac{c^2}{op} = .154;$$

and since the portion due to skill = .142, we may infer that .923 of this success is due to skill, and only .077 to chance. On the other hand, of his success in predicting the non-occurrence of tornadoes, only .147 is due to skill, and .853 is due to chance.

Prophecy and fulfillment are effects of a common cause. Neither causes the other. The problem, broadly stated, requires a numerical expression for the causal relation between two classes of phenomena either in co-existence or in sequence, when the presence of one corresponds sometimes to the presence and sometimes to the absence of the other, and sometimes both are absent. In case of sequence it is immaterial which is antecedent. The quantities denoted by o and p should therefore be interchangeable.

My formula responds properly to every test proposed by Mr. Gilbert. The value of i increases rapidly with that of c, and

slowly with that of s, diminishes with increase of o or p, and varies between the limits 0 and 1. Skill in making false predictions is indicated by a negative value of $cs - op$; but the same degree of causal relation exists as when equal skill is employed in making true predictions; and a negative value of i can never occur. When $s =$ either p or o, $i = \frac{0}{0}$; but the apparent indeterminateness vanishes when we consider that i is the product of two factors, of which one $= 0$ and the other is indeterminate within limits. And the value of i is unaltered when predictions of non-occurrences are substituted for those of occurrences, and *vice versa.* In the latter case, write $s - o$ for o, $s - p$ for p, and $s - o - p + c$ for c; and the formula reduces to its original form.

In addition to Mr. Gilbert's tests, two others may be considered. In the case of predictions all falsely reported, we may write $s - p$ for p and $o - c$ for c; and the formula becomes

$$i = \frac{(op - cs)^2}{op\,(s - o)\,(s - p)},$$

with a proper reversal of signs in the quantity under the exponent and no change in the value of i.

If occurrences always appear whenever they are not predicted, and never appear when they are predicted, we put $c = 0$ and $p = s - o$, with the result

$$i = 1;$$

or the logical connection is perfect.

In order that the general formula shall be properly applicable, care must be taken that the predictions are fairly homogeneous in definiteness of time and space. For illustration: if predictions that phenomena will occur in given months are examined indiscriminately with those that they will occur on given days, the result will be manifestly worthless.

It has been proposed to extend the problem so as to include three or more classes of events of which one must happen and only one can happen in any case. It seems clear to me that no single numerical expression can be a proper solution of such a problem. Suppose the three classes of events, A, B, and C. By the method above given A and Not A may be examined; and all instances

involving either the prediction or occurrence of A may be excluded and B and C separately investigated. Suppose it thus ascertained that great skill has been shown in discriminating between A and Not A, and little or none in discriminating between B and C. No single numerical expression can properly comprehend these heterogeneous results.

Mr. CURTIS showed that some of the results given by Mr. Doolittle could be independently deduced by another method.

Mr. GILBERT noted as a defect in the formula proposed by Prof. Peirce, that it did not duly discourage positive predictions of rare events; and, while gratified with Mr. Doolittle's discussion of the subject, he expressed a disappointment that no satisfactory decision as to the treatment of cases of three or more alternatives had been reached by him.

After some further discussion, a communication by Mr. M. BAKER was called, but postponed, on motion of Mr. H. FARQUHAR, to allow time for the consideration of a testimonial to a late associate, Mr. ALVORD.

Mr. E. B. ELLIOTT read the following tribute, prepared by Mr. BAKER and himself:

MEMORIAL.

The Mathematical Section of the Philosophical Society of Washington, having suffered the loss by death, on October 16th, 1884, of General BENJAMIN ALVORD, one of its founders and active workers, desires to place on record this testimonial to his worth and to the loss to this Section and to science by his death.

Of his worth, one of America's greatest mathematicians has said that he was a scientist of "real originality who had actually extended the boundaries of science."

The bent of General Alvord's mind and studies was early directed towards a purely geometrical solution of the general problem of tangencies, and his reward, which it is our pleasure to chronicle, was success.

Of his mathematical publications, the following is submitted as a provisionally complete list:

LIST OF MATHEMATICAL PUBLICATIONS BY GENERAL BENJAMIN ALVORD.

1. The tangencies of circles and of spheres.
   [*In* Smithsonian Contributions to Knowledge. 4°. Washington, 1856, Vol. 8, Article 4, 16 pp., 9 plates.]
   Also issued separately.

2. On the interpretation of imaginary roots in questions of maxima and minima.
   [*In* The Mathematical Monthly. 4°. New York, 1860, April, Vol. 2, No. 7, pp. 237–240.]

3. Tangencies.
   [*In* Johnson's New Universal Cyclopædia. 8°. New York, 1878, Vol. 4, pp. 723–4.]

4. Mortality in each year among the officers of the army for fifty years, from 1824 to 1873, as derived from the army registers.
   [*In* Proceedings of the American Association for the Advancement of Science, 23d Meeting, Hartford, August, 1874. 8°. Salem, 1875, pp. 57–59.]

5. The intersection of circles and the intersection of spheres.
   [*In* American Journal of Mathematics. 4°. Baltimore, 1882, March, Vol. 5, No. 1, pp. 25–44; 4 plates.]

6. Curious fallacy as to the theory of gravitation.
   [*In* Bulletin of the Philosophical Society of Washington. 8°. Washington, 1883, Vol. 5, pp. 85–88.]

7. A special case in maxima and minima.
   [*In* Bulletin of the Philosophical Society of Washington. 8°. Washington, 1884, Vol. 6, p. 149.]

Mr. M. Baker, in moving the adoption of this memorial by the Section, said:

General Alvord's entire life was that of the soldier, and his routine of life work did not call him in the direction of mathematical study. Hence whatever he accomplished in mathematics or literature was accomplished in military surroundings and with only such facilities as barrack and camp life afford. If under these

conditions the total of his contributions to science appears small, we should bear in mind that any contribution under such circumstances is exceptional. And to have been able, therefore, to make even a single contribution to human knowledge is to have done that which few men in any generation do and that of which any one of us might well be proud.

General Alvord early became interested in the problem of tangencies and intersections of circles, and his chief mathematical work and fame rests on his complete and purely geometrical solution of the various problems relating to this subject. His chief writings on this subject consist of the paper on Tangencies, in the Smithsonian Contributions in 1856; the article on Tangencies, in Johnson's New Universal Cyclopædia; and the paper on intersections, in the American Journal of Mathematics, March, 1882.

The memorial was adopted, and the Secretary was instructed to send a copy of it to the family of the deceased.

Note.

The following members have assisted the Chairman and Secretary in the examination of abstracts of communications to the Mathematical Section:

Title.	*Author.*	*Third Member.*
The Problem of the Knight's Tour	G. K. Gilbert.	E. B. Elliott.
Formulæ for Diminution of Amplitude of a Pendulum	H. Farquhar.	A. S. Christie.
The Formulæ for Computing the Position of a Satellite	A. Hall.	C. H. Kummell.
The Quadric Transformation of Elliptic Integrals	C. H. Kummell.	G. W. Hill.
The Verification of Predictions	M. H. Doolittle.	M. Baker.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. VIII.

Containing the Minutes of the Society and of the Mathematical Section for the year 1885.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON:
1885

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WASHINGTON, D. C.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

CONSTITUTION, RULES,

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AND REPORTS OF

SECRETARIES AND TREASURER.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

---

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who, having complied with Section 14 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. * On request of a member, the President or either of the Secretaries may, at his discretion, issue to any person a card of invitation to attend a specified meeting. Five cards of invitation to attend a meeting may be issued in blank to the reader of a paper at that meeting.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

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* Amended January 17, 1885.

11. Any paper read before a Section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

12. *It is not permitted to report the proceedings of the Society or its Sections for publication, except by authority of the General Committee.

13. New members may be proposed in writing by three members of the Society for election by the General Committee; but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

14. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

15. The fiscal year shall terminate with the Annual Meeting.

16. †Any member who is absent from the District of Columbia for more than twelve consecutive months may be excused from payment of dues during the period of his absence, in which case he will not be entitled to receive announcements of meetings or current numbers of the Bulletin.

17. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

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* Adopted January 17, 1885. † Amended December 19, 1885.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 13 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. If the author of any paper read before a Section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee to be appointed as the Section may determine.

The report of this committee shall be forwarded to the Publication Committee by the Secretary of the Section, together with any action of the Section taken thereon.

6. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 20, 1884.

President	ASAPH HALL.	
Vice-Presidents	J. S. BILLINGS.	GARRICK MALLERY.
	WILLIAM HARKNESS.	J. E. HILGARD.
Treasurer	ROBERT FLETCHER.	
Secretaries	G. K. GILBERT.	HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

MARCUS BAKER.
H. H. BATES.
F. W. CLARKE.
W. H. DALL.
C. E. DUTTON.
J. R. EASTMAN.
E. B. ELLIOTT.
H. M. PAUL.
C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* G. K. GILBERT. HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman.* ROBERT FLETCHER. HENRY FARQUHAR. S. F. BAIRD.*

* As Secretary of the Smithsonian Institution.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 19, 1885.

President	J. S. BILLINGS.	
Vice-Presidents	WILLIAM HARKNESS.	GARRICK MALLERY.
	C. E. DUTTON.	J. E. HILGARD.
Treasurer	ROBERT FLETCHER.	
Secretaries	G. K. GILBERT.	MARCUS BAKER.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

H. H. BATES.	F. W. CLARKE.
W. H. DALL.	C. E. DUTTON.
J. R. EASTMAN.	HENRY FARQUHAR.
T. C. MENDENHALL.	H. M. PAUL.

C. V. RILEY.

STANDING COMMITTEES.

On Communications:

WILLIAM HARKNESS, *Chairman*. G. K. GILBERT. MARCUS BAKER.

On Publications:

G. K. GILBERT, *Chairman*. ROBERT FLETCHER. MARCUS BAKER.
S. F. BAIRD.*

* As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to January 16, 1886.

The customary titles of members are here for the first time given.

Names of gentlemen here indicated as resigned will be omitted from future lists.

Name.	Address and Residence.	Admitted.
ABBE, Prof. CLEVELAND	Army Signal Office. 2017 I st. N. W.	1871
ABERT, Mr. S. T. (Sylvanus Thayer)	810 19th st. N. W.	1875
ADAMS, Mr. CHARLES FREDERICK	Civil Service Commission. 2017 G st. N. W.	1885
ADAMS, Mr. HENRY	1607 H st. N. W.	1881
ALDIS, Hon. A. O. (Asa Owen)	1765 Mass. ave. N. W.	1873
ANTISELL, Dr. THOMAS (*Founder*)	Patent Office. 1311 Q st. N. W.	1871
AVERY, Mr. ROBERT S. (Robert Stanton)	320 A st. S. E.	1879
BAIRD, Prof. SPENCER F. (Spencer Fullerton) (*Founder*)	Smithsonian Institution. 1445 Mass. ave. N. W.	1871
BAKER, Dr. FRANK	326 C st. N. W.	1881
BAKER, Mr. MARCUS	Coast and Geodetic Survey Office. 1205 Rhode Island ave.	1876
BANCROFT, GEORGE	1623 H st. N. W., or Newport, R. I.	1875
BARNARD, Dr. WM. S. (William Stebbins)	917 New York ave. N. W., or Canton, Ill.	1884
BARUS, Dr. CARL	Geological Survey.	1885
BATES, Mr. HENRY H. (Henry Hobart)	Patent Office. The Portland.	1871
BEARDSLEE, Capt. L. A. (Lester Anthony) U. S. N. (*Absent*)	Navy Department.	1875
BELL, Mr. A. GRAHAM (Alexander Graham)	Scott Circle; 1500 Rhode Island ave.	1879
BELL, Dr. C. A. (Chichester Alexander)	1314 19th st. N. W.	1881
BENÉT, Gen. S. V. (Stephen Vincent) U. S. A. (*Founder*)	Ordnance Office, War Dept. 1717 I st. N. W.	1871

Name.	Address and Residence.	Admitted.
Bessels, Dr. Emil	Glenn Dale, Md.	1875
Billings, Dr. John S. (John Shaw) U. S. A. (*Founder*)	Surg. General's Office, U. S. A. 3027 N st. N. W.	1871
Birney, Gen. William	456 Louisiana ave. 1901 Harewood ave., Le Droit Park.	1879
Birnie, Rogers (*Absent*)		1876
Bodfish, Mr. S. H. (Sumner Homer) (*Absent*)	Geological Survey. 605 F st. N. W.	1883
Boutelle, Mr. C. O. (Charles Otis)	Coast and Geodetic Survey Office.	1884
Bowles, Asst. Nav. Constr. Francis T. (Francis Tiffany) U. S. N.	Navy Department.	1884
Browne, Dr. J. Mills (John Mills) U. S. N.	Navy Department. The Portland.	1883
Burchard, Hon. H. C. (Horatio Chapin) (*Resigned*)		1879
Burgess, Mr. E. S. (Edward Sanford)	High School. 810 12th st. N. W.	1883
Burnett, Dr. Swan M. (Swan Moses)	1215 I st. N. W.	1879
Busey, Dr. Samuel C. (Samuel Clagett)	901 16th st. N. W.	1874
Casey, Col. Thomas Lincoln, U. S. A. (*Founder*)	War Department. 1419 K st. N. W.	1871
Caziarc, Lieut. L. V. (Louis Vasmer) U. S. A. (*Absent*)	War Department.	1882
Chamberlin, Prof. T. C. (Thomas Crowder)	Geological Survey. 939 K st. N. W.	1883
Chatard, Dr. Thomas M. (Thomas Marean)	Geological Survey. 601 13th st. N. W.	1885
Chickering, Prof. J. W., Jr. (John White)	Deaf Mute College, Kendall Green.	1874
Christie, Mr. Alex. S. (Alexander Smyth)	Coast and Geodetic Survey Office. 507 6th st. N. W.	1880
Clark, Mr. E. (Edward)	Architect's Office, Capitol. 417 4th st. N. W.	1877
Clarke, Prof. F. W. (Frank Wigglesworth)	Geological Survey. 1425 Q st. N. W.	1874
Coffin, Prof. J. H. C. (John Huntington Crane) U. S. N. (*Founder*)	1901 I st. N. W.	1871
Comstock, Prof. J. H. (John Henry) (*Absent*)	Cornell University, Ithaca, N.Y.	1880
Coues, Prof. Elliott	Smithsonian Institution. 1726 N st. N. W.	1874
Craig, Lieut. Robert, U. S. A. (*Absent*)	1008 I st. N. W.	1873
Craig, Dr. Thomas (*Absent*)	Johns Hopkins Univ., Baltimore, Md.	1879

NAME.	ADDRESS AND RESIDENCE.	Admitted.
CURTIS, Mr. GEO. E. (George Edward)	Army Signal Office. 930 18th st. N. W.	1884
DALL, Mr. WM. H. (William Healey) (*Founder*)	Care Smithsonian Institution. 1119 12th st. N. W.	1871
DAVIS, Commander C. H. (Charles Henry) U. S. N.	Navy Department. 1705 Rhode Island ave.	1880
DEAN, Dr. R. C. (Richard Crain) U. S. N. (*Absent*)	Naval Hospital, New York city.	1872
DE CAINDRY, Mr. Wm. A. (William Augustin)	Commissary General's Office. 1713 H st. N. W.	1881
DELAND, Mr. THEODORE L. (Theodore Louis)	Treasury Department. 126 7th st. N. E.	1880
DEWEY, Mr. FRED. P. (Frederic Perkins)	National Museum. Lanier Heights.	1884
DILLER, Mr. J. S. (Joseph Silas)	Geological Survey. 1804 16th st. N. W.	1884
DOOLITTLE, Mr. M. H. (Myrick Hascall)	Coast and Geodetic Survey Office. 1925 1 st. N. W.	1876
DUNWOODY, Lt. H. H. C. (Henry Harrison Chase) U. S. A. (*Absent*)	War Department.	1873
DUTTON, Capt. C. E. (Clarence Edward) U. S. A.	Geological Survey. 2222 G st. N. W.	1872
EARLL, Mr. R. EDWARD (Robert Edward)	Smithsonian Institution. 1336 T st. N. W.	1884
EASTMAN, Prof. J. R. (John Robie) U. S. N.	Naval Observatory. 1823 I st. N. W.	1871
EIMBECK, Mr. WILLIAM	Coast and Geodetic Survey Office.	1884
ELDREDGE, Dr. STEWART (*Absent*)	Yokohama, Japan.	1871
ELLIOTT, Mr. E. B. (Ezekiel Brown) (*Founder*)	Gov't Actuary, Treas. Dept. 1210 G st. N. W.	1871
EMMONS, Mr. S. F. (Samuel Franklin)	Geological Survey. 1022 Vermont ave.	1883
ENDLICH, Dr. F. M. (Frederic Miller) (*Absent*)	Smithsonian Institution. Lake Valley, New Mexico.	1873
EWING, Gen. HUGH (*Absent*)	Lancaster, Ohio.	1874
FARQUHAR, Mr. EDWARD	Patent Office Library. 1915 H st. N. W.	1876
FARQUHAR, Mr. HENRY	Coast and Geodetic Survey Office. Brooks Station, D. C.	1881
FERREL, Prof. WILLIAM	Army Signal Office. 471 C st. N.W.	1872
FLETCHER, Dr. ROBERT	Surgeon General's Office, U. S. A. The Portland.	1873
FLINT, Mr. A. S. (Albert Stowell)	Naval Observatory. 1450 Chapin st., College Hill.	1882
FLINT, Dr. J. M. (James Milton) U. S. N.	Navy Department. U. S. S. Albatross.	1881
FRISTOE, Prof. EDWARD T	Columbian University. 1434 N st. N.W.	1873

NAME.	ADDRESS AND RESIDENCE.	Admitted.
GALLAUDET, President E. M. (Edward Miner)	Deaf Mute College, Kendall Green.	1875
GANNETT, Mr. HENRY	Geological Survey. 1881 Harewood ave., Le Droit Park.	1874
GIHON, Dr. ALBERT L. (Albert Leary) U. S. N. (*Resigned*)		1880
GILBERT, Mr. G. K. (Grove Karl)	Geological Survey. 1424 Corcoran st.	1873
GODDING, Dr. W. W. (William Whitney)	Government Hospital for the Insane.	1879
GOOCH, Dr. F. A. (Frank Austin)	Geological Survey. 825 Vermont ave.	1885
GOODE, Mr. G. BROWN (George Brown)	National Museum. 1545 T st. N. W.	1874
GOODFELLOW, Mr. EDWARD	Coast and Geodetic Survey Office.	1875
GORE, Prof. J. H. (James Howard)	Columbian University. 1305 Q st. N. W.	1880
GRAVES, Mr. WALTER H. (Walter Hayden) (*Absent*)	Denver, Colorado.	1878
GREELY, Lieut. A. W. (Adolphus Washington) U. S. A.	Army Signal Office. 1914 G st N. W.	1880
GREEN, Mr. BERNARD R. (Bernard Richardson)	1738 N st. N. W.	1879
GREEN, Commander F. M. (Francis Mathews) U. S. N. (*Absent*)	Navy Department.	1875
GREENE, Prof. B. F. (Benjamin Franklin) (*Founder: absent*)	West Lebanon, N. H.	1871
GREENE, Capt. FRANCIS V. (Francis Vinton) U. S. A. (*Absent*)	West Point, N. Y.	1875
GREGORY, Dr. JOHN M. (John Milton)	15 Grant Place.	1884
GUNNELL, FRANCIS M., M. D., U. S. N.	600 20th st., N. W.	1879
HAINS, Col. PETER C. (Peter Conover)	Engineer's Office, Potomac Riv. Improvement, 2136 Pa. ave. 1824 Jefferson Place.	1879
HAINS, Mr. ROBERT P. (Robert Peter)	Patent Office. 1714 13th st. N. W.	1885
HALL, Prof. ASAPH, U. S. N. (*Founder*)	Naval Observatory. 2715 N st. N. W.	1871
HALL, Mr. ASAPH, JR.	Yale College Observatory, New Haven, Conn. 2715 N st. N. W.	1884
HALLOCK, Dr. WILLIAM	Geological Survey.	1885
HAMPSON, Mr. THOMAS	Geological Survey. 504 Maple ave., Le Droit Park.	1885
HARKNESS, Prof. WILLIAM, U. S. N. (*Founder*)	Naval Observatory. Cosmos Club, 23 Madison Place.	1871

NAME.	ADDRESS AND RESIDENCE.	Admitted.
HASSLER, Dr. FERDINAND A. (Ferdinand Augustus) (*Absent*)	Santa Aña, Los Angeles Co., Cal.	1880
HAYDEN, Dr. F. V. (Ferdinand Vandeveer) (*Founder: absent*)	Geological Survey. 1805 Arch st., Phila., Pa.	1871
HAZEN, Prof. H. A. (Henry Allen)	P. O. Box No. 427. 1416 Corcoran st.	1882
HAZEN, Gen. W. B. (William Babcock) U. S. A.	Army Signal Office. 1601 K st. N. W.	1881
HEAP, Maj. D. P. (David Porter)	Light House Board, Treas. Dept. 1618 Rhode Island ave.	1884
HENSHAW, Mr. H. W. (Henry Wetherbee)	Bureau of Ethnology. 13 Iowa Circle.	1874
HILGARD, Mr. J. E. (Julius Erasmus) (*Founder*)	1739 F st. N. W.	1871
HILL, Mr. G. W. (George William)	Nautical Almanac Office. 314 Indiana ave. N. W.	1879
HITCHCOCK, Mr. ROMYN	National Museum, or P. O. Box 630.	1884
HODGKINS, Prof. H. L. (Howard Lincoln)	Columbian University. 627 N st. N. W.	1885
HOLDEN, Prest. EDWARD SINGLETON (*Absent*)	University of California, Berkeley, Cal.	1873
HOLMES, Mr. W. H. (William Henry)	Geological Survey. 1100 O st. N. W.	1879
HOWELL, Mr. EDWIN E. (Edwin Eugene) (*Absent*)	48 Oxford st., Rochester, N. Y.	1874
IDDINGS, Mr. JOSEPH P. (Joseph Paxson)	Geological Survey. 1028 Vermont ave.	1885
JAMES, Rev. OWEN (*Absent*)	Scranton, Pa.	1880
JENKINS, Rear Admiral THORNTON A. (Thornton Alexander) U. S. N. (*Founder*)	2115 Penna. ave. N. W.	1871
JOHNSON, Mr. A. B. (Arnold Burges)	Light House Board, Treas. Dept. 501 Maple ave., Le Droit Park.	1878
JOHNSON, Dr. JOSEPH TABER	926 17th st. N. W.	1879
JOHNSON, Mr. WILLARD D. (Willard Drake) (*Absent*)	Geological Survey.	1884
JOHNSTON, Dr. W. W. (William Waring)	1603 K st. N. W.	1873
KAUFFMANN, Mr. S. H. (Samuel Hays)	1000 M st. N. W.	1884
KEITH, Mr. R. (Reuel)	Nautical Almanac Office. 2219 I st. N. W.	1871
KERR, Mr. MARK B. (Mark Brickell)	Geological Survey. 722 21st st. N. W.	
KIDDER, Dr. J. H. (Jerome Henry)	Smithsonian Institution. 1816 N st. N. W.	1880

Name.	Address and Residence.	Admitted.
Kilbourne, Lt. C. E. (Charles Evans) U. S. A. (*Absent*)	War Department.	1880
King, Dr. A. F. A. (Albert Freeman Africanus)	726 13th st. N. W.	1875
Knox, Hon. John Jay (*Absent*)	Prest. Nat. Bank Republic, New York city	1874
Kummell, Mr. C. H. (Charles Hugo)	Coast and Geodetic Survey Office. 608 Q st. N. W.	1882
Lawrence, Mr. William	1344 Vermont ave., and Bellefontaine, Ohio.	1884
Lawver, Dr. W. P. (Winfield Peter)	Mint Bureau, Treas. Dept. 1912 I st. N. W.	1881
Lee, Dr. William	2111 Penna. ave. N. W. 1821 I st. N. W.	1874
Lefavour, Mr. Edward B. (Edward Brown)	905 O st. N. W.	1882
Lincoln, Dr. N. S. (Nathan Smith)	1514 H st. N. W.	1871
Loomis, Mr. E. J. (Eben Jenks)	Nautical Almanac Office. 1413 Stoughton st. N. W.	1880
Lull, Capt. E. P. (Edward Phelps) U. S. N. (*Absent*)	Navy Department. 74 Cedar st., Roxbury, Mass.	1875
McGee, Mr. W J	Geological Survey. 1424 Corcoran st.	1883
McGuire, Mr. Fred. B. (Frederick Bauders)	1416 F st. N. W. 614 E st. N. W.	1879
McMurtrie, Prof. William (*Absent*)	University of Illinois, Champaign, Ill.	1876
Maher, Mr. James A. (James Arran)	Geological Survey. 21 E st. N. W.	1884
Mallery, Col. Garrick, U. S. A.	Bureau of Ethnology. 1323 N st. N. W.	1875
Mann, Mr. B. Pickman (Benjamin Pickman)	Department of Agriculture. 924 19th st. N. W.	1885
Marcou, Mr. J. B. (John Belknap)	Geological Survey. 601 13th st. N. W.	1884
Marvin, Prof. C. F. (Charles Frederick)	Army Signal Office. 1736 13th st. N. W.	1885
Marvin, Mr. Jos. B. (Joseph Badger) (*Absent*)	Internal Revenue Bureau.	1878
Mason, Prof. Otis T. (Otis Tufton)	National Museum. 1305 Q st. N. W.	1875
Matthews, Dr. W. (Washington) U. S. A.	Surg. General's Office, U. S. A.	1884
Meigs, Gen. M. C. (Montgomery Cunningham) U.S.A. (*Founder*)	1239 Vermont ave.	1871
Mendenhall, Prof. T. C. (Thomas Corwin)	Army Signal Office.	1885
Merrill, Mr. George P. (George Perkins)	National Museum.	1884

Name.	Address and Residence.	Admitted.
Morgan, Dr. E. C. (Ethelbert Carroll)	918 E st. N. W.	1883
Moser, Lt. J. F. (Jefferson Franklin) U. S. N.	Coast and Geodetic Survey Office. 7 2d st. S. E.	1885
Murdoch, Mr. John	Smithsonian Institution. 1441 Chapin st., College Hill.	1884
Mussey, Gen. R. D. (Reuben Delavan)	P. O. Box 618. 508 5th st. N. W.	1881
Myers, Gen. William, U. S. A.	War Department.	1871
Newcomb, Prof. Simon, U. S. N. (*Founder*)	Navy Department. 941 M st. N. W.	1871
Nichols, Dr. Charles H. (Charles Henry) (*Absent*)	Bloomingdale Asylum, Boulevard and 117th st., New York, N. Y.	1872
Nicholson, Mr. W. L. (Walter Lamb) (*Founder*)	Topographer, P. O. Dept. 1322 I st. N. W.	1871
Nordhoff, Mr. Charles	1731 K st. N. W.	1879
Norris, Dr. Basil U. S. A. (*Absent*)	Vancouver, Clarke Co., Wash. Ter.	1884
Nott, Judge C. C. (Charles Cooper)	Court of Claims. 826 Connecticut ave. N. W.	1885
Ogden, Mr. Herbert G. (Herbert Gouverneur)	Coast and Geodetic Survey Office. 1324 19th st. N. W.	1884
Osborne, Mr. J. W. (John Walter)	212 Delaware ave. N. E.	1878
Parke, Gen. John G. (John Grubb) U. S. A. (*Founder*)	Engineer Bureau, War Dept. 16 Lafayette Square.	1871
Parker, Dr. Peter (*Founder*)	2 Lexington Place.	1871
Parry, Dr. Charles C. (Charles Christopher) (*Absent*)	Davenport, Iowa.	1871
Paul, Mr. H. M. (Henry Martyn)	Naval Observatory. 109 1st st. N. E.	1877
Peale, Dr. A. C. (Albert Charles)	Geological Survey. 1010 Mass. ave. N. W.	1874
Pilling, Mr. James C. (James Constantine) (*Resigned*)		1881
Poe, Gen. O. M. (Orlando Metcalfe) U. S. A. (*Absent*)	34 Congress st. West, Detroit, Mich.	1873
Poindexter, Mr. W. M. (William Mundy)	701 15th st. N. W. 806 17th st. N. W.	1884
Pope, Dr. B. F. (Benjamin Franklin) U. S. A.	Surg. General's Office, U. S. A.	1882
Powell, Major J. W. (John Wesley)	Geological Survey. 910 M st. N. W.	1874
Prentiss, Dr. D. W. (Daniel Webster)	1224 9th st. N. W.	1880
Pritchett, Prof. H. S. (Henry Smith) (*Absent*)	Director of Observatory, Wash. University, St. Louis, Mo.	1879

Name.	Address and Residence.	Admitted.
Rathbun, Mr. Richard	Smithsonian Institution. 1622 Mass. ave.	1882
Ravené, Mr. Gustave L. (Gustave Louis)	1417 6th st. N. W.	1885
Ray, Lieut. P. H. (Patrick Henry) U. S. A.	Fort Gaston, Cal.	1884
Renshawe, Mr. Jno. H. (John Henry)	Geological Survey. The Woodmont.	1883
Richey, Dr. S. O. (*Resigned*)		1882
Ricksecker, Mr. Eugene	Geological Survey. 1323 Q st. N. W.	1884
Riley, Dr. C. V. (Charles Valentine)	Agricultural Department, or National Museum. 1700 13th st. N. W.	1878
Ritter, Mr. W. F. McK. (William Francis McKnight)	Nautical Almanac Office. 16 Grant Place.	1879
Robinson, Mr. Thomas	Howard University. 6th st. N. W., cor. Lincoln.	1884
Rogers, Mr. Joseph A. (Joseph Addison) (*Absent*)	Naval Observatory.	1872
Russell, Mr. Israel C. (Israel Cook)	Geological Survey. 1424 Corcoran st.	1882
Russell, Mr. Thomas	Army Signal Office. 1447 Corcoran st. N. W.	1883
Salmon, Dr. D. E. (Daniel Elmer)	Agricultural Department. 1337 15th st. N. W.	1883
Sampson, Commander William Thomas U. S. N. (*Absent*)	Torpedo Station, Newport, R. I.	1883
Saville, Mr. J. H. (James Hamilton)	1419 F st. N. W. 1315 M st. N. W.	1871
Schott, Mr. Charles A. (Charles Anthony) (*Founder*)	Coast and Geodetic Survey Office. 212 1st st. S. E.	1871
Shellabarger, Hon. Samuel	Room 23 Corcoran Building. 812 17th st. N. W.	1875
Sherman, Hon. John	U. S. Senate. 1319 K st. N. W.	1874
Shufeldt, Dr. R. W. (Robert Wilson) U. S. A. (*Absent*)	Surg. Genl's Office, U. S. A., or Box 144, Smithsonian Inst.	1881
Shumway, Mr. W. A. (Willard Adams) (*Resigned*)		1885
Sicard, Capt. Montgomery, U. S. N.	Ordnance Bureau, Navy Dept.	1877
Sigsbee, Commander C. D. (Charles Dwight) U. S. N. (*Absent*)	Navy Department.	1879
Skinner, Dr. J. O. (John Oscar) U. S. A.	Surg. General's Office, U. S. A. 1529 O st. N. W.	1883
Smiley, Mr. Chas. W. (Charles Wesley)	U. S. Fish Commission. 943 Mass. ave.	1882
Smith, Chf. Eng. David, U. S. N.	1330 Corcoran st.	1876

Name.	Address and Residence.	Admitted.
Smith, Mr. Edwin	Coast and Geodetic Survey Office. 2024 Hillyer Place.	1880
Spofford, Mr. A. R. (Ainsworth Rand)	Library of Congress. 1621 Mass. Ave. N. W.	1872
Stearns, Mr. Robert E. C. (Robert Edwards Carter)	Smithsonian Institution. 1635 13th st. N. W.	1884
Stone, Prof. Ormond (*Absent*)	Leander McCormick Observatory, University of Virginia, Va.	1874
Taylor, Mr. F. W. (Frederick William) (*Absent*)	Lake Valley, New Mexico.	1881
Taylor, Mr. Wiliam B. (William Bower) (*Founder*)	Smithsonian Institution. 306 C st. N. W.	1871
Thompson, Prof. A. H. (Almon Harris)	Geological Survey.	1875
Thompson, Mr. Gilbert	Geological Survey. 1448 Q st. N. W.	1884
Todd, Prof. David P. (David Peck) (*Absent*)	Lawrence Observatory, Amherst, Mass.	1878
Toner, Dr. J. M., (Joseph Meredith)	615 Louisiana ave.	1873
True, Mr. Frederick W. (Frederick William)	National Museum. 1335 N st. N. W.	1882
Upton, Mr. Wm. W. (William Wirt)	1416 F st. N. W. 1746 M st. N. W.	1882
Upton, Prof. Winslow (*Absent*)	Brown University, Providence, R. I.	1880
Walcott, Mr. C. D. (Charles Doolittle)	Geological Survey; National Museum.	1883
Waldo, Prof. Frank (*Absent*)	Army Signal Office, Fort Myer, Va.	1881
Walker, Mr. Francis A. (Francis Amasa) (*Absent*)	Massachusetts Institute of Technology, Boston, Mass.	1872
Walling, Mr. Henry F. (Henry Francis) (*Absent*)	U. S. Geological Survey, Cambridge, Mass.	1883
Ward, Mr. Lester F. (Lester Frank)	Geological Survey. 1464 Rhode Island ave.	1876
Webster, Mr. Albert L. (Albert Lowry) (*Absent*)	West New Brighton, Staten Island, N. Y.	1882
Weed, Mr. Walter H. (Walter Harvey)	Geological Survey. 1413 Rhode Island ave.	1885
Welling, Mr. James C. (James Clarke)	1302 Connecticut ave.	1872
Wheeler, Capt. Geo. M., U. S. A.	Lock Box 92.	1873
White, Dr. C. A. (Charles Abiather)	Geological Survey. 312 Maple ave., Le Droit Park	1876
White, Dr. C. H. (Charles Henry) U. S. N.	Museum of Hygiene, 1744 G st. N. W.	1884
Williams, Mr. Albert, Jr.	Geological Survey.	1883

NAME.	ADDRESS AND RESIDENCE.	Admitted.
WILLIS, Mr. BAILEY	Geological Survey. 2017 N st. N. W.	1885
WILSON, H. M. (Herbert Michael)	Geological Survey.	1885
WILSON, Mr. J. ORMOND (James Ormond)	1439 Massachusetts ave. N. W.	1873
WINLOCK, Mr. WILLIAM C. (William Crawford)	Naval Observatory. 718 21st st. N. W.	1880
WOOD, Mr. JOSEPH (*Absent*)	Supt. Motive Power, Penn Co., Fort Wayne, Ind.	1875
WOOD, Lt. W. M. (William Maxwell) U. S. N. (*Absent*)	Navy Department.	1871
WOODWARD, Mr. R. S. (Robert Simpson)	Geological Survey. 1125 17th st. N. W.	1883
WORTMAN, Dr. J. L. (Jacob Lawson)	Army Medical Museum. 916 14th st. N. W.	1885
WRIGHT, Mr. GEO. M. (George Mitchell)	Geological Survey. 1319 Vermont ave.	1885
YARROW, Dr. H. C. (Harry Crécy)	Surgeon General's Office, U. S. A. 814 17th st. N. W.	1874
YEATES, Mr. W. S. (William Smith)	Smithsonian Institution. 1008 E st. S. W.	1884
ZIWET, Mr. ALEXANDER	Coast and Geodetic Survey Office. 1456 Corcoran st.	1885
ZUMBROCK, Dr. A. (Anton)	455 C st. N. W.	1875

LIST OF DECEASED MEMBERS.

Name.	*Admitted.*
Benjamin Alvord	1872
Orville Elias Babcock	1871
Theodorus Bailey	1873
Joseph K. Barnes	Founder
Henry Wayne Blair	1884
Horace Capron	Founder
Salmon Portland Chase	Founder
Frederick Collins	1879
Benjamin Faneuil Craig	Founder
Charles Henry Crane	Founder
Josiah Curtis	1874
Richard Dominicus Cutts	1871
Charles Henry Davis	1874
Frederick William Dorr	1874

Name.	*Admitted.*
Alexander B. Dyer	Founder
Amos Beebe Eaton	Founder
Charles Ewing	1874
Elisha Foote	Founder
John Gray Foster	1873
Leonard Dunnell Gale	1874
Isaiah Hanscom	1873
Joseph Henry	Founder
Franklin Benjamin Hough	1879
Andrew Atkinson Humphreys	Founder
Ferdinand Kampf	1875
Washington Caruthers Kerr	1883
Jonathan Homer Lane	Founder
Oscar A. Mack	1872
Archibald Robertson Marvine	1874
Fielding Bradford Meek	Founder
James William Milner	1874
Albert J. Myer	Founder
George Alexander Otis	Founder
Carlile Pollock Patterson	1871
Titian Ramsay Peale	Founder
Benjamin Peirce	Founder
John Campbell Riley	1877
John Rodgers	1872
Benjamin Franklin Sands	Founder
George Christian Shaeffer	Founder
Henry Robinson Searle	1877
William J. Twining	1878
Joseph Janvier Woodward	Founder
John Maynard Woodworth	1874
Mordecai Yarnall	1871

SUMMARY.

Active members	179
Absent members	46
Total	225
Deceased members	45

ANNUAL REPORT OF THE SECRETARIES.

WASHINGTON, CITY, *December* 19, 1885.

To the Philosophical Society of Washington:

We have the honor to present the following statistical data for 1885.

At the beginning of the year the number of active members was	173
This number has been increased by the addition of 22* new members and by the return of 3 absent members. It has been diminished by the departure of 6 members, by the death of 1, by the resignation of 5, and by the dropping of 7 for non-payment of dues. The net increase of active members has thus been	6
And the active membership is now	179

The roll of new members is:

C. F. ADAMS.	H. L. HODGKINS.	W. A. SHUMWAY.
CARL BARUS.	J. P. IDDINGS.	W. H. WEED.
T. M. CHATARD.	B. P. MANN.	BAILEY WILLIS.
F. A. GOOCH.	C. F. MARVIN.	H. M. WILSON.
R. P. HAINS.	T. C. MENDENHALL.	J. L. WORTMAN.
WILLIAM HALLOCK.	J. F. MOSER.	G. M. WRIGHT.
THOMAS HAMPSON.	C. C. NOTT.	ALEX. ZIWET.
	G. L. RAVENÉ.	

The names of deceased members (active and absent) are:

HORACE CAPRON. F. B. HOUGH. W. C. KERR. T. R. PEALE.

There have been 16 meetings for the presentation and discussion of papers (not including the public meeting of Dec. 5); the average attendance has been 48. There have been 6 meetings of the Mathematical Section; average attendance 15.

In the general meetings 32 communications have been presented; in the mathematical section 14. Altogether 46 communications have been made by 32 members and one guest. The number of members who have participated in the discussions is 41. The total number who have contributed to the scientific proceedings is 54, or 31 per cent. of the present active membership.

The General Committee has held 17 meetings. Attendance below 10 on one evening only, and four times as high as 15. Average 12.2. Corresponding average last year 11.9, and in 1883 10.3, when the attendance was below 10 at five meetings.

Very respectfully,

G. K. GILBERT,
HENRY FARQUHAR,
Secretaries.

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* The figures in this report have been brought down to Jan. 16, 1886, so as to correspond with the list of members. They differ somewhat from those read to the Society at the annual meeting.

REPORT OF THE TREASURER.

Mr. President and Gentlemen:

The report which I have the honor to present to you to-night covers the pecuniary transactions of the fiscal year which terminates with this meeting.

You will see from the statement which I shall presently read that the total receipts by the Treasurer have been $1,224.04, and the total disbursements $740.02, leaving a cash balance of $484.02. These sums do not, however, represent the net income and expenditure for the year, since the receipts include a balance transferred by the former Treasurer, and the amount collected of unpaid dues of previous years. The payments, also, include the sum of $104.12, for unpaid debts of 1884.

All the liabilities of the Society have been discharged to date, and the actual income belonging to 1885 has been $1,041.00, which includes $205.00 of dues yet outstanding. The disbursements for the same period have been $635.90, leaving a net saving for the current year of $405.10.

The unpaid dues of former years which have been collected, amount to $160.00.

The Government bonds which belong to the Society were at the beginning of the year exchanged for new issues of the same amounts and denominations. This was done, under authority of the General Committee, in order to obtain uniformity in the designation of the Society, the former bonds having varied from each other in that particular. The bonds consist of:—

1	$1,000 00	bond,	bearing	interest	at	4	per cent.
1	500 00	"	"	"		4	"
1	1,000 00	"	"	"		4½	"

Total, $2,500 00.

The assets of the Society are as follows:—

Cash, with Riggs & Co.	$482 02
Bonds	2,500 00
Unpaid dues	295 00
Total	$3,279 02

The market value of the bonds is, of course, considerably in excess of their face value; on the other hand, a part of the "unpaid dues" will probably not be collected.

I have in my possession a still ample stock of the Bulletins. A copy of Vol. VII was sent directly after its publication in February last to every member entitled to receive it, as well as to the various societies and scientific journals, at home and abroad, with which it has been customary for the Society to exchange publications.

DR. *The Treasurer in account with The Philosophical Society of Washington.* CR.

1885.				
Jan.	5.	To cash received from C. Abbe		$213 04
Mar.	20.	" " for sale of Bulletin		1 00
Dec.	19.	" " dues of 1882		5 00
		" " " 1883		25 00
		" " " 1884		130 00
		" " " 1885		730 00
		" " " 1886		15 00
		" " for interest on bonds: —		
		On $1,000 at 4½ per cent.	$45 00	
		" 1,500 at 4 "	60 00	
				105 00
				$1,224 04

1885.		By disbursements, as per vouchers: —	
March	17.	Cash paid Judd & Detweiler, bal. found due on last year's account.	$40 00
		Cash paid same for printing, binding, etc. vol. 7 of the Bulletin	500 63
June	13.	Cash paid janitor for attendance on 18 meetings of the Society	18 00
Dec.	19.	Cash paid for miscellaneous printing, circulars, bills, postal cards, and new set of record and account books	69 15
		Cash paid miscellaneous expenses of Secretaries and Treasurer, for postage, stationery, clerical work, etc.	112 24
Balance, with Riggs & Co.			484 02
			$1,224 04

ROBERT FLETCHER,

Treasurer.

CALENDAR FOR THE USE OF THE PHILOSOPHICAL SOCIETY,

Showing the alternate SATURDAYS for holding Meetings during the several "Seasons" from 1884–'85 to 1907–'08, inclusive.

PREPARED BY MR. E. B. ELLIOTT.

Submitted to the General Committee June 7, 1884, and ordered published.

Years.	October.	November.	December.	Years.	January.	February.	March.	April.	May.	June.
1884	11, 25	8, 22	6, 20	1885	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1885	10, 24	7, 21	5, 19	1886	2, 16, 30	13, 27	13, 27	10, 24	8, 22	5, 19
1886	9, 23	6, 20	4, 18	1887	15, 29	12. 26	12, 26	9, 23	7, 21	4, 18
1887	15, 29	12, 26	10, 24	1888	7, 21	4, 18	3, 17, 31	14, 28	12, 26	9, 23
1888	13, 27	10, 24	8, 22	1889	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1889	12, 26	9, 23	7, 21	1890	4, 18	1, 15	1, 15, 29	12, 26	10, 24	7, 21
1890	11, 25	8, 22	6, 20	1891	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1891	10, 24	7, 21	5, 19	1892	2, 16, 30	13, 27	12, 26	9, 23	7, 21	4, 18
1892	15, 29	12, 26	10, 24	1893	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1893	14, 28	11, 25	9, 23	1894	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1894	13, 27	10, 24	8, 22	1895	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1895	12, 26	9, 23	7, 21	1896	4, 18	1, 15, 29	14, 28	11, 25	9, 23	6, 20
1896	10, 24	7, 21	5, 19	1897	2, 16, 30	13, 27	13, 27	10, 24	8, 22	5, 19
1897	9, 23	6, 20	4, 18	1898	15, 29	12, 26	12, 26	9, 23	7, 21	4, 18
1898	15, 29	12, 26	10, 24	1899	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1899	14, 28	11, 25	9, 23	1900	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1900	13, 27	10, 24	8, 22	1901	5, 19	2, 16	2, 16, 30	13. 27	11, 25	8, 22
1901	12, 26	9, 23	7, 21	1902	4, 18	1, 15	1, 15, 29	12, 26	10, 24	7, 21
1902	11, 25	8, 22	6, 20	1903	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1903	10, 24	7, 21	5, 19	1904	2, 16, 30	13, 27	12, 26	9, 23	7, 21	4, 18
1904	15, 29	12, 26	10, 24	1905	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1905	14, 28	11, 25	9, 23	1906	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1906	13, 27	10, 24	8, 22	1907	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1907	12, 26	9, 23	7, 21	1908	4, 18	1, 15, 29	14, 28	11, 25	9, 23	6, 20

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ANNUAL ADDRESS OF THE PRESIDENT

ANNUAL ADDRESS OF THE PRESIDENT,

ASAPH HALL.

Delivered December 5, 1885.

AMERICAN SCIENTIFIC SOCIETIES.

Mr. Chairman and Fellow-Members of the Philosophical Society:

The termination of the office with which you have honored me during the past year brings, in accordance with our custom, the duty of addressing you, and I have chosen for my subject *American Scientific Societies.* The Philosophical Society of Washington is the first scientific society of which I was a member, and, having still a lively recollection of the curiosity and interest with which I watched its formation and early progress, I propose to consider briefly the history of such societies in our own country, and incidentally, some of their benefits.

Nothing can be more natural than the union of men of similar tastes and thought into associations for the investigation and discussion of matters that mutually interest them, and thus we see in all civilized countries the formation of societies in every branch of learning and of art. In the countries of Europe such bodies have been a long time in existence, and many of them are still in vigorous life. Most of these societies owe their establishment to the favor of a powerful patron, generally an emperor or a king, who was wise enough to understand that the well-being of his people would be enhanced by the progress of science and art. But whatever may have been the motive of their foundation, these academies of scientific men have exerted a great influence on the civilization of Europe. Such an assertion may seem doubtful to the readers of what is called history, but in fact the larger part of our civilization that is good and permanent will be found closely connected with the works and inventions of scientific men. It is these works that have changed our ideas and conception of the world in which we live, and of the universe around us. It is these works, also, that slowly but surely compel the changes of political and theological theories. History, as it is now written, deals mostly with battles and sieges, with the

actions of emperors and kings, the amours of princes, and the intrigues of courtiers and priests. These are the bubbles and froth of the social world that have attracted nearly all historical writers, but there is a growing feeling that such recitals do not contain the substantial parts of history. Who would not wish to know more of the social forces that have been at work in Europe and in our own country, and which have converted some of the most rugged and barren parts of the world into the richest and most prosperous—of the ingenious and persevering industry that brings about the greatest changes in the customs of men and in the political power of nations? These changes come from an increase of command over the forces of nature, and in our ability to make these forces work for us. Now it is the discovery and study of these forces and their modes of action that form a large part of scientific work; and if we turn to the academies of Europe we shall find that they have a splendid history. By their encouragement of the labors and writings of ingenious men, by just criticism, and by the publication of memoirs these academies take a prominent place in the history of science. It is in these enduring monuments of the human intellect, rather than in brazen statues or marble shafts, that the real glory of the race consists.

The men who settled our country were separated by a great ocean and a month's journey from the civilization and learning of Europe. They had ample work to do in building houses and roads, and in establishing themselves securely in this new world. However, they soon began to set up schools and colleges, so that the elements at least of learning might be kept alive. But the inhabitants were scattered over a great extent of country, and means of communication were poor. Under these circumstances concerted action and union into societies were difficult, and a century and a half passed away before the formation of a formal scientific society. The first society of this kind that I find in our country is the American Philosophical Society of Philadelphia, organized in 1769. At that time Philadelphia was the largest and the leading city of this country, and being in a good degree free of the bitter theological quibbles and disputes that embroiled New England communities, it was better adapted for the home of a scientific society. It was also the residence of Benjamin Franklin, who appears to have taken an active part in the formation of the new society, and who became its first president. Franklin was a native of Boston, but when seven-

teen years of age, having written and spoken disrespectfully of what he called "religious knaves," and having thus provoked the enmity of influential men in his native town, he sought and found a home in a more genial climate. The new society began well. Its first three presidents were Benjamin Franklin, David Rittenhouse, and Thomas Jefferson. This society formulated and published an excellent plan of scientific work, including a study of the native inhabitants of the country, an examination of the ancient mounds of the Western States, and researches in geology and natural history. In 1796 it offered several prizes, with premiums ranging from fifty to one hundred dollars. The first premium offered was "for the best system of liberal education and literary instruction adapted to the genius of the government, and best calculated to promote the general welfare of the United States, comprehending, also, a plan for instituting and conducting public schools in this country on principles of the most extensive utility." Premiums were also offered for improving the method of computing longitudes from lunar distances, for the improvement of ship pumps, for the improvement of stoves or fire-places, for the best method to prevent the premature decay of peach trees, for a treatise on native American vegetable dyes, and for the best improvement of lamps. This society published its first volume of memoirs in 1771. Among the first contributors was David Rittenhouse, the able and ingenious astronomer, and the first volume contains a very full account of the Transit of Venus that happened June 3, 1769. We find here, also, some account of improvements in the sextant, which appears to have been independently invented by Thomas Godfrey, of Philadelphia, in 1730. Franklin was an early contributor to the memoirs, his writings generally having a very practical bearing. His first paper is on the causes and cure of smoky chimneys, and occupies thirty-six quarto pages. It is an interesting paper on an important subject, much discussed at that time. A correspondent of Franklin declares its importance by quoting the lines—

> "A smoky house and a scolding wife
> Are two of the greatest ills in life."

This was followed by other papers of Franklin on the formation of the earth, the theory of light and heat and of magnetism, and on the manufacture of paper. Franklin thought the interior of the earth is a heavy fluid, and he imagined magnetism to be a general

property of the universe, so that one might govern his course from star to star by the compass. The succeeding volumes contain several ingenious memoirs by Joseph Priestley, in some of which he expounds the theory of phlogiston, which appears to have been purely a hypothetical substance for explaining the theory of combustion. The volume of these memoirs published in 1825 contains a letter from Albert Gallatin, Secretary of the Treasury, under date of March 25, 1807, and addressed to Mr. F. R. Hassler, of Philadelphia, relating to the survey of our coast. There is a long and interesting reply by Hassler, who gives a full account of the methods of such a survey, with descriptions of instruments, forms for keeping the observations, making maps, and carrying on the work generally. Still later these volumes contain the memoirs of Joseph Henry on his important researches in electricity and magnetism. These papers were read in January, 1835, and published in 1837. The American Philosophical Society has published twenty-one volumes of memoirs, which contain papers of enduring interest. One feels regret that a society that began so well and which has published so much of value should stop the publication of its memoirs and seem to flag in its scientific work. Let us hope that this may be only a temporary condition.

The next establishment of a scientific society in our country is that of the American Academy of Arts and Sciences in Boston. This society was chartered in 1780, James Bowdoin being the first president. The Boston society does not appear to have started under such favorable auspices as its sister society in Philadelphia, but on the other hand it has kept up its scientific work better, and is still active and efficient. Perhaps this may be owing to its proximity to a large and flourishing college, which has now developed into a university. The American Academy of Arts and Sciences has published fifteen volumes of memoirs. Among its distinguished members I cannot omit to mention Nathaniel Bowditch, the self-taught mathematician, who was probably the first man in our country to really grasp the methods of the *Mecanique Celeste.* It is one of the surprises of our prolific country that it produces so many men who, it is said, read the *Mecanique Celeste* before they graduate from college, and it is another surprise to meet these same men in after life and be convinced from their own lips that they know but little about that great work. But Nathaniel Bowditch mastered it.

These two societies, that of Philadelphia and that of Boston, are

the most widely known and they are among the best of our local societies. Many others have been established or revived recently, and some of them are doing good work. Among those which have taken a high standing by the publication of valuable memoirs is the Connecticut Academy of Arts and Sciences. Such societies deserve a hearty support, and by their encouragement and direction of local talent can render valuable service to science. But these societies are too numerous to mention here.

I cannot, however, pass from the two elder societies without noticing in the first place the gradual cessation of their memoirs and the falling into what are styled "proceedings" for publication. It seems to me a matter of regret that this should happen. Such publications are apt to degenerate into a dry account of meetings, and elections, and deaths and resignations, and lists of members. If this is all a society is able to do it may be tolerated for awhile, but it is a condition which should be outgrown. I think that keeping up a good form for printing memoirs tends to elevate the character of a society and to incite members to good works.

There is another matter in connection with these two elder societies which is curious and worthy of mention. Each of them had a list of foreign honorary members. It is interesting now after the lapse of a century to examine these lists, and to see what kind of men were selected for such honors, and also to see how far the judgment of the philosophers has been confirmed by time, which makes such havoc with the estimates of men. At the time of the organization of the American Philosophical Society, Euler was the leading mathematician of Europe. He was then sixty-two years old and at the height of his reputation. There is hardly a branch of mathematics which Euler had not enriched by his ingenious and wonderfully prolific labors. He had worked in the theory of numbers, in all parts of the calculus, and had laid the foundations of the calculus of variations. He wrote a complete treatise on dioptrics, made a laborious and valuable investigation of the lunar theory, and applied mathematical theories to a very great number of physical questions. In 1766 he became blind by his incessant labors, but still continued his work. I do not find the name of Leonard Euler in the list of the fifty-five foreign associates of the American Philosophical Society. The successor of Euler as the leading mathematician of the world was Lagrange. The *Mecanique Analytique* was not published until 1788, but Lagrange had shown his power in a great

number of memoirs on mathematical and astronomical questions. The *Mecanique Analytique* put him at the head of all living investigators in the theory of rational mechanics; and this book remains to-day a model worthy the study of every student. In fact at the present time, when so many doctors and professors seize the great analytical machine and turn out pages of elegant and trifling formulæ, there can be no better experience than to go back to the luminous pages of Lagrange. Here is a master whose symbols are always in subjection, and who has no need to startle us by mysterious phrases. His inventions and improvements are among the most valuable in the history of pure and applied mathematics. But the name of Lagrange is not on the list of honorary members of the American Philosophical Society. Laplace was a younger man than Lagrange, but he was fortunate in securing a good position in Paris in early life, and he immediately began that wonderful career of scientific labors that culminated in the *Mecanique Celeste.* The first volume of this work was published in the year *seven*, or in 1799. It is the methodical arrrangement and condensation of the labors of his great predecessors and colleagues of that century, together with his own remarkable investigations—a work that placed him at the head of the philosophers of his day. But his fame was not of the kind to put him on the honorary list of the American Philosophical Society. It may be worth while to look for one name more, that of Legendre, the ingenious and persevering mathematician who lived such a quiet and unpretentious life that we are not surprised to find his name omitted. I think this example is worthy of notice, since it shows that the scales of philosophers do not weigh men much more accurately than those of other people. But it would be a mistake to suppose that this society differed in this respect very much from others of its own time. The matter may be instructive in warning us against careless estimates of our cotemporaries, and by giving us caution in judging scientific men from their social position and rank. It may also throw light on the election to positions in our scientific societies of men whose chief recommendation is a noisy and uncertain reputation. However these things may be one thing is certain: the lords, counts, and gentlemen of the honorary lists are dead, buried, and forgotten; but the names of the four men who could not command this distinction live in the memories of all men of science.

Omitting for the present any reference to the many scientific socie-

ties that have been organized recently I come to our national societies. The earliest of these is the American Association for the Advancement of Science. The first meeting of this association was held in Philadelphia in 1848, and was called to order by Professor William B. Rogers, of Virginia, who had taken a prominent part in its formation. Only one volume of memoirs has been published by the American Association, but its annual volume of proceedings has been issued with regularity. This association has embraced among its members nearly all the prominent scientific men of the country, and it is our most complete national scientific organization. Theplan of its formation seems to have been a good one, and I think it has exerted an excellent influence by bringing into acquaintance and sympathy men from different parts of the country. In recent years its character has become more popular, and under the lead of its energetic secretary its membership has reached nearly two thousand. During its early days this society took an active part in discussing the scientific operations carried on by the General Government, and its influence in this direction seems to have been wise. With the increase in the number of members such discussions have been judiciously avoided, and even the passing of resolutions, so common in all American bodies, might perhaps better be omitted. In such large bodies there is apt to be so much confusion and dispute that the resolutions are made extremely vague and meaningless or are manipulated to suit the purpose of a few. There is another danger to this society arising from its easy conditions to membership and its rapid increase of members. Our country produces a large number of men and women who are born with a mission. Educated in the schools and colleges, but never attaining much distinction as scholars, these people begin in their own phrase to think for themselves. The result of this thinking is often some discovery in science, and one that contravenes doctrines established by long observation and study. The questions considered are generally vast and mysterious, such as the origin of gravitation, the nebular hypothesis, and the nature of force. Having made his discovery the author wishes of course to present it to the world, and what method is more convenient than through a scientific society, admission to which is so easy. And if we admit a person to membership and take his money how can we refuse to listen to his theories. Who that has had the honor of presiding over one of the sections of the American Association, in casting his eye over

the audience, has not had brought to mind the description of Dean Swift:

"The first man I saw was of meagre aspect, with sooty hands and face; his hair and beard long, ragged, and singed in several places.
. . .
He had been eight years upon a project for extracting sunbeams out of cucumbers, which were to be put into phials hermetically sealed, and let out to warm the air in raw and inclement summers. He told me he did not doubt in eight years more he should be able to supply the governor's gardens with sunshine at a reasonable rate; but he complained that his stock was low, and he entreated me to give him something as an encouragement to ingenuity, especially as this had been a dear season for cucumbers."

Now although this matter has a comical phase, it has also its serious and difficult side. No man of science wishes to suppress the opinions of others, and ingenious speculations are worthy of attention, but he has a right to his own time, and should be freed from the trouble of listening to absurd projects. How this can be done with such an easy course of admission to membership I do not see. There is another hindrance to the successful operation of the American Association which comes from the great extent of our country and the cost and difficulty of attending its meetings. To those who have ample time and means at their disposal this hindrance is not, perhaps, very great. This vigorous and generous society may need a little pruning, but on the whole its influence has been good, and every one must wish it a long and honorable life.

We have another scientific organization of national character in the National Academy of Sciences, established in 1863. This is a body on a basis quite different from that of the American Association for the Advancement of Science. The National Academy was brought into existence during a great civil war, and its members were of necessity chosen from one section of the country. It was incorporated by act of Congress, and this act limited the number of its members to fifty. From the language of the act we may fairly infer that the academy was intended to be the adviser of the General Government in matters of science. During a time of great civil commotion, when the powers of the Government were greatly extended, such a society would very naturally come into existence; but when the strife had subsided it became an object of criticism.

In filling vacancies in its membership it was difficult always to

select the best man, and sometimes abler men were left out of the academy than those who had the right kind of influence to be appointed. Scientific men are like other people, and they elect their friends and those whom they think will help them. Within a few years after the close of the civil war the limitation of the number of members was removed by act of Congress. The National Academy has now the power to determine the number of its members, and for the present this has been practically fixed at one hundred. Whether this number should be increased, and whether its membership should be more evenly distributed throughout the country are questions over which the academy has entire control. Its destiny, therefore, is in its own hands, and it is to be hoped, and it is to be expected, that its career will be useful and honorable. To act such a part as this the academy must maintain a high and independent character. It should choose for its members the best and ablest scientific men of the country, and it must never become the tool of the shrewd men who deal out the rich patronage of the Government. That there is need for such an independent body to criticise and assist in the direction of the scientific work done by public authority seems beyond question. It is assumed, of course, that the General Government is to carry on scientific works of various kinds, a position which may be disputed by some, but which appears to be already practically conceded. How far the Government should enter on such works, and how much should be left to private enterprise, is a question of public policy. But there are certain works which belong almost of necessity to the General Government. Thus the survey of our coast and harbors, a general geological survey, and a good map of the country seem to belong to the work of the Government. These may be justified on the ground of their utility to the public. But there are other scientific works, not so directly connected with commercial and moneyed interests, that an enlightened government may properly undertake. Why should not there be in this country a first-class national astronomical observatory, where observations may be continued from one age to another with the best instruments of the times? Again, do not the elevated plains of the West offer an excellent opportunity for the determination of an arc of the meridian which may be extended from British America to the City of Mexico, and why cannot our General Government undertake such a determination? Must everything that is not strictly utilitarian be prohibited in the public works of our Repub-

lic? I do not think so. On the other hand the things that are purely commercial may generally be left to themselves.

But if the Government is to do scientific work it should have the aid of men of science. We all know the tendency of public officials to fall into habits of routine, and to spin out their work to an almost interminable length. There should be, therefore, a body of men who can criticise kindly, but boldly and justly, the labors of officials, and make them perform their duties as they ought. This, I think, should be one of the functions of the National Academy of Sciences. But to make this advice influential the Government must recognize the scientific men of the country, and give them some regular channel of communication through which their opinions can be made known to the public and to the executive authorities. Thus far in the history of our Government the scientific man has generally been regarded as an expert who is to be carefully watched, lest he get the better of the officials who are set over him, and who sometimes undertake to manage affairs of which they have but little knowledge. The jealousy thus engendered is unfortunate. The man of science should be treated just as other men are treated, and there should be no grumbling at paying him a fair recompense for his labor. Our national military and naval academies are costly institutions, and fortunate is the young man who has a Congressman for an uncle or a cousin; but I have never heard a word from any scientific man against the cost of these establishments. So far as I know, their universal sentiment is, let us have the best of instruction in military and naval science, for this is the cheapest. Our public buildings cost vast sums of money, but there is no objection to such expenditures if the buildings are well and solidly constructed, since here, also, the best is the cheapest. On the other hand, is there not something very absurd in the manner the politician looks on the small expenditures for science, and the lavish ones that are voted for other purposes? Let us take a single case. A public vessel is "repaired," to use an adopted euphemism, at one of our navy yards, and the cost of the repairs amounts to a million of dollars, or more than double the original cost of the ship. There is some astonishment at this, but we are told in a confident manner that the ship is greatly improved in strength and speed. The trial comes off, and while the ship is going along at her utmost speed, with the velocity of nine miles an hour, the engine breaks down. Here is a serious collapse, since before

she was repaired the vessel could make eleven miles an hour. The head of the Department very properly orders an examination. Now, it might occur to superficial persons that those who repaired that ship had made mistakes. But the examining board weighs the evidence carefully, and it deliberately comes to the conclusion that the fault lies wholly with some unknown person who is more than three thousand miles away. The matter is mysterious, but the result is certain. The Government has been cheated, the money is gone, and the local politicians are happy. And the curious thing is that the public accepts the report of the examining board as entirely satisfactory. There is not a whisper of dissent from any newspaper in the land. And, after all, it is only half a million dollars, and do we not throw away ten times as much every year on rivers and harbors? Have we not seen a Senator boasting in his speeches that during the last twenty years more of the public money has been expended on the mountain streams of his own State than that State has paid taxes into the public treasury.

Now change the case and let us suppose that some scientific man by bad management of his own or by failure of an assistant, has wasted ten thousand dollars of the public money. Ah, this is quite a different matter, and must be looked at from a different standpoint. How soon do we hear some smug official complacently remark that he always knew that scientific men cannot do business. And how eagerly the newspapers seize upon the case; how indignant the editors become, and how the head lines flare. Is there one law for the public functionary and another for the man of science?

But it is not right to leave this matter without further consideration. When we look at the advantageous position occupied by the officer of the army or navy we see immediately that this does not come from any personal merit he may have, but from the fact that he is recognized by law as an essential part of the Government. This position renders him in theory impersonal, and it is assumed that he has no private business of his own, but all his interests are one with those of the public. He has his member of the Cabinet to represent his views. His appropriations never fail, and he has no need to summon men from distant parts of the country to push his bills through Congress. Now, so long as the scientific man is looked upon merely as an expert and an adventurer, and has no regular channel of communication with the Government he will

stand at a great disadvantage He may gain a victory now and then, just as militia may sometimes beat regular troops, but the final result is pretty sure to be defeat. The position of the man of science must, therefore, be recognized by law if he is to be connected with public works in such a way that he may act freely and usefully. Such considerations will bring up the question of how far the Government is to proceed in the cultivation of science.

If we examine the history of a country like England, where we have good records for a thousand years, we shall see that there has been a steady tendency toward three results. The first of these is personal liberty. The slave that was bought and sold has been changed to the serf, and the serf to the laborer. These changes have gone on with conflicts, and sometimes with retrograde movements, but on the whole they have proceeded until now, in nearly all civilized countries, personal liberty is secured by law. The second result is the freedom of opinion. To control such an intangible thing as the opinions of men is a difficult matter, but it is a business which many men delight in, and the contest, though old, is yet a living one. When Sir Richard Saltonstall reproached his friends in Boston for persecuting Baptists and Quakers, on the ground that such persecutions made men hypocrites, the Puritan ministers at once replied that hypocrites are much better than profane persons like Baptists and Quakers. But such people have been forced back from one position to another, explaining, apologizing, and retreating, until now in several countries opinion is nearly free. There remain a few able men who pray for more superstition and bigotry, but they are the relics of a past time. The third result is the right of free exchange, and toward this end we have gained but little, since nearly all governments exercise their power in prohibiting among men the free exchange of their products. The general course of events is thus to restrict the sphere of government, and to leave to the individual more and more freedom of action. The chief duty of government is to see that justice is done between man and man, and to this end that the courts are fair and intelligent, and that our judges are not owned by rich men and corporations; that the public service is honest and efficient, and is not used for personal or political aggrandizement. But, granting all this, it seems to me that the Government may properly undertake such great scientific works as I have mentioned, with the condition that they be placed under proper control and inspection. To the successful accomplishment

of these works the man of science must be brought into the public service. He must be held responsible, just as other officials are, in his account of public money. Such a condition would subject him to some limitations that might be irksome at first, since it is pleasant for many to have at their disposal large sums of money which they may use at their pleasure, and almost every one fancies that he could do a great deal of good in this way. But such a method of handling public money is dangerous, and is apt to lead into trouble.

Supposing that our public scientific works are to be carried on by men of science, what part the National Academy of Sciences shall act I cannot say, and it is not my province to urge on the Government the services of this academy, but here is a body of scientific men who have pledged themselves to the public service and they should be made to do their duty. And is there not ample room for intelligent criticism and suggestion in all the methods through which the public money is expended? Take the case of public schools, which is a kind of communism justified on the ground of utility to the State. What shall be the course of education in these schools? There is an unfortunate class of our fellow creatures that must be cared for at the public expense, but shall benevolent institutions encourage the production of such beings? Have we not read of the English poorhouse where were found the grandfather, the father, and the son, all hearty men,—paupers breeding paupers? Do not some of our charitable institutions give plausibility to the saying that the mistakes of the good do more harm than the vices of the wicked? In fact, turn to any of the modes of public expenditure and examples will be found where sober, scientific judgment is necessary for the wise conduct of business.

I come now to consider our own home society, the Philosophical Society of Washington. And, speaking cautiously and soberly, is it not to-day the best local scientific society in the country? This is owing partly and perhaps chiefly to our position at the Capital of a great people. Men from all sections come back to us as winter approaches, and many of them have interesting information to give. No other city of our country offers such advantages for fresh and early information of the investigations that are going on in the various departments of science. Our libraries in astronomy, mathematics, and medical science are among the best. As a general

library that of Congress must surpass all others in the country. These are advantages which make Washington an agreeable residence for men of science and literature.

I think also our society has a good plan of organization, thanks to the men who formed it. The general business can be safely confided to a committee, and in this way the meetings are made more interesting. This committee is so large that by a generous rotation in office most of the members may see and share, if they wish, the governing of the society. We have simple rules and they should always be enforced, since no society can afford to be overawed by any man, especially a society where we all meet as equals and where no favors are to be asked or granted. Our society has been established on a broad basis, to include all brances of learning, and as we have members from all the professions our meetings ought not to lack in variety of subjects. In such meetings the purpose of a paper should be to present the principal points clearly, and the author may generally trust to the intelligence of his audience to fill in the details. It is the failure to recognize this rule and the lack of arrangement that make some papers so long and tedious.

Our society has its home in a beautiful city, and who that has seen its wonderful growth during recent years can doubt its future splendor and greatness? It is a city cosmopolitan in its character. Being the seat of political power, here will come the enterprising and adventurous from all parts of the country, with additions from other lands. Some of the brightest names in our scientific annals are those of foreigners who have made their homes with us. Let us welcome all earnest men, remembering that the principles of science are universal, and are not confined to any language or country.

In respect of personal conduct we can have no better example than the noble man who was our first President, whose simple and devoted life was a model for every scientific man. If we need other inducements to devote ourselves to labors that may not give a great return of money, or lead to easy and luxurious lives, let us remember that we live in a magnificent country, and one that has been dedicated as we hope to the liberty and welfare of the human race. Each one of us may do a little in adding to her scientific renown, which is now only beginning. Let us recall the words of the great Athenian: "I would have you day by day fix your eyes upon the greatness of Athens, until you become filled with the love of her; and when you are impressed by the spectacle of her glory, reflect

that this empire has been acquired by men who knew their duty and had the courage to do it, who in the hour of conflict had the fear of dishonor always present to them, and who, if ever they failed in an enterprise, would not allow their virtues to be lost to their country, but freely gave their lives to her as the fairest offering which they could present at her feast. The sacrifice which they collectively made was individually repaid to them; for they received again each one for himself a praise which grows not old, and the noblest of all sepulchres—I speak not of that in which their remains are laid, but of that in which their glory survives, and is proclaimed always and on every fitting occasion both in word and deed. For the whole earth is the sepulchre of famous men; not only are they commemorated by columns and inscriptions in their own country, but in foreign lands there dwells also an unwritten memorial of them, graven not on stone but in the hearts of men."

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

GENERAL MEETING.

BULLETIN

OF THE

GENERAL MEETING.

261ST MEETING. JANUARY 3, 1885.

President HALL in the Chair.

The Chair announced the election to membership of Messrs. WILLIAM MUNDY POINDEXTER and ASAPH HALL, Jr.

The Auditing Committee, appointed at the annual meeting, submitted the following report:

WASHINGTON, D. C., *December* 27, 1884.

Mr. President and Gentlemen of the
Philosophical Society of Washington :

We, your committee, appointed at the annual meeting, December 20, 1884, to audit the report of the Treasurer for the year 1884, have the honor to submit the following report:

We have examined the statement of receipts of dues from members, and of interest on bonds, and find the former to be $745, and the latter $95, as appears in the Treasurer's statements of accounts for the year 1884.

In addition to the foregoing, $15 were repaid to the Treasurer by a member for extra printing, thus making the total receipts $855.

We have examined the vouchers for disbursements for the same period, and find them correct.

We have compared the return checks with the vouchers and with the entries in the bank book, and find them correct.

We have examined the bank book, and found the balance as set forth to be correct, said balance, deducting the amount of two checks not yet returned, being $183.04, with Messrs. Riggs & Co.

The bonds referred to in the statement of assets were exhibited to us by the Treasurer, and consist of one $1,000 U. S. bond @ 4½ per cent., one $1,000 U. S. bond @ 4 per cent., and one $500 U. S. bond also @ 4 per cent.

All of which is respectfully submitted.

H. C. YARROW,
MARCUS BAKER,
WILLIAM C. WINLOCK,
Committee.

On motion, the report was accepted and the committee discharged.

Mr. J. S. BILLINGS made a communication on

THE VITAL STATISTICS OF THE TENTH U. S. CENSUS,

presenting a brief outline of results soon to be published in Vol. XI of the Census Reports.

Remarks were made by Messrs. ELLIOTT, MALLERY, and ASAPH HALL.

262D MEETING. JANUARY 17, 1885.

The President in the Chair.

The Chair communicated an invitation to the members of the Society to attend the annual meeting of the Biological Society and listen to an address by Dr. C. A. WHITE.

By request, Professor G. STANLEY HALL, of the Johns Hopkins University, made a communication on

RECENT EXPERIMENTS ON REACTION TIME, AND THE TIME SENSE,

reviewing the methods of investigation and the results attained.

An animated discussion followed by Messrs. ROBINSON, HILGARD, NEWCOMB, BILLINGS, EASTMAN, PAUL, WINLOCK, ASAPH HALL, and Professor GEORGE DAVIDSON, of San Francisco.

Mr. C. E. DUTTON then began a communication on

PRACTICAL GEOLOGY *versus* SPECULATIVE PHYSICS,

which was unfinished when the hour of adjournment arrived.

263D MEETING. JANUARY 31, 1885.

The President in the Chair.

Fifty-eight members and guests present.

The Chair communicated an invitation to the members of the Society to attend the ninetieth regular meeting of the Anthropological Society, and listen to an address by Major J. W. POWELL.

Mr. C. E. DUTTON finished the communication begun by him at the last meeting on

PRACTICAL GEOLOGY *versus* SPECULATIVE PHYSICS,

and the subject was further discussed by Messrs. DOOLITTLE, MASON, CLARKE, WARD, WALCOTT, PAUL, TAYLOR, M. BAKER, and ROBINSON.

264TH MEETING. FEBRUARY, 14, 1885.

The President in the Chair.

Thirty-one members present.

The Chair announced the election to membership of Messrs. CARL BARUS, FRANK AUSTIN GOOCH, and WILLIAM HALLOCK.

Messrs. F. W. CLARKE and J. S. DILLER made a joint communication on

TOPAZ FROM STONEHAM, MAINE,

describing the alteration of topaz into damourite. [The paper is published in the American Journal of Science, 3d series, Vol. XXIX, pp. 378–384.]

Mr. WILLIAM H. DALL made a communication on

TWO REMARKABLE FORMS OF MOLLUSKS.

Mr. DALL described the anatomical features of the remarkable *Chlamydoconcha Orcutti*, of San Diego, California,* which is in fact a degraded lamellibranch, in which the shell has become internal and functionless, and is no longer adjusted by adductor muscles.

He also described a remarkable feature which he had just discovered in *Milneria minima*, a small California bivalve, belonging

*The main features of this notice appear in Science, No. 76, Vol. IV, p. 50, 1884.

to the *Carditidæ*, and living on the backs of *Haliotis* shells. The female has the base of the shell pushed up into a dome in the median line, the opening to which is closed by an extension of the mantle. In the pocket so formed the young of the species are protected by the mother. The only other case among the lamellibranchs of such a protective modification is that of *Thecalia concamerata*, in which the same end is reached in a different manner. Both belong to the same family. The males of *Milneria* are without the pocket. Both sexes adhere by a byssus.

Mr. W. B. TAYLOR read a communication on

GEOLOGICAL AND PHYSICAL THEORIES,

in which, controverting the claims of practical or field geology—to the exclusion of physical theory—in the solution of physiographic problems, he contended that the family ties of planetary relationship cannot be disowned by geology. He thought the value of "external" inductions fully shown by the probable effects of varying eccentricity in the earth's orbit on secular changes of climate, as well as by a reference to the general inter-relation between the meeting boundaries of astronomical, geological, physical, and chemical science. On the physical side, he maintained that the supposed demonstrations of the earth's comparatively recent consolidation, (as well as of the limit assigned to the sun's active life,) were entirely inconclusive: first, from the admitted uncertainty of the data, and secondly, from our ignorance that unknown factors might not enter into the problem. He therefore heartily agreed with Captain Dutton in recognizing the strong demands of geological induction for an incomparably longer chronology than terrestrial physics could as yet cipher out.* At the same time, the speaker contended that the certainty remained entirely unimpaired of an origin and a limit to solar—as well as to planetary—energy; unless we were prepared to accept the absurdity of an infinite potential. He also pointed out that the doctrine of "uniformitarianism" does not require (as sometimes too readily supposed) an unvarying degree of energy in geological dynamics throughout the distant past; but that the contrary was the more probable—if only from the broad generalization that all action whatever has its period or periods of maximum and minimum.

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* Mr. Taylor's paper was a reply to one by Mr. Dutton, of which the Society obtained no abstract. See pp. 4 and 5.

On the question whether geology itself gave us "traces of a beginning, or prospects of an end," Mr. T. argued that stratigraphical geology unmistakably indicated its own genesis in the plutonic character of its primæval "Archæan," or Laurentian—pointing to a time when the primitive surface was a molten ocean; and that when read in the light of palæontology such indication of a beginning was strengthened into convincing proof by the receding gradations of animal and vegetable life, starting in the lower Silurian and its underlying Cambrian, with the humblest invertebrate forms of molluscan and crustacean life, and the simplest cryptogamous thallogens—the marine algæ and fucaceæ. And in this connection he referred to the memorable generalization of Louis Agassiz—that the geological successions of animal types correspond remarkably with the phases of embryonic development—as one of the most suggestive contributions ever made to the theory of evolution.

The speaker then turned to the question of the earth's interior fluidity; and after stating that the celebrated mathematical arguments of Hopkins from the "precession" value, and of Thomson from the hydrographic tides, had both been practically abandoned by the latter—though he still persisted in his pre-possessions for a solid globe (mainly on the specific-gravity skepticism), Mr. T. said he felt no difficulty whatever in accepting the geological evidences of a fluid earth enveloped by a flexile, friable egg-shell. With regard to the large amount of contraction and corrugation every where exhibited by this shell, he admitted that Mr. O. Fisher had conclusively disproved the sufficiency of Élie de Beaumont's plausible hypothesis that the contraction is due to the secular cooling of the planet. Mr. Fisher had however no better speculation to offer; and the answer to the riddle must come ultimately—not from petrology; nor from structural, or stratigraphical, or physiographical geology—but from cosmological physics. In conclusion, the speaker urged that the same inductions which so clearly establish the birth, the childhood, and the manhood of our planet, as inevitably implicate its decline, decadence, and decease; and he quoted passages from Byron's familiar lines on "Darkness," as in the main a scientific prophecy.

Mr. Paul spoke of the importance of a recent contribution to the subject of the earth's rigidity by Mr. George H. Darwin. Mr. Gilbert thought that Darwin's deduction of high rigidity was vitiated by his postulate of homogeneity.

The President remarked on the great interest of the discussion opened by Captain DUTTON's communication to a preceding meeting, and expressed his especial approval of the method in which Mr. WARD had approached the subject.

265TH MEETING. FEBRUARY 28, 1885.

The President in the Chair.

Fifty members and guests present.

The Chair announced the election to membership of Messrs. THOMAS CORWIN MENDENHALL, ALEXANDER ZIWET, HOWARD LINCOLN HODGKINS, BAILEY WILLIS, JOSEPH PAXSON IDDINGS, and C. F. MARVIN.

Announcement was also made of the death of the Hon. HORACE CAPRON.

Mr. C. ABBE made a communication on

METHODS OF VERIFYING WEATHER PREDICTIONS,

giving a general account of the rules under which the U. S. Signal Office deduces from "indications" and subsequent observations the published percentages of verifications. For purposes of prediction and verification the area of the United States is divided into a small number of districts. The "indication" for each district refers to the subsequent 24 hours, and is compared with the three next following weather-maps constructed from the observations, and the degree of correspondence for each station in the district is marked on a scale of five terms—0, 25, 50, 75, and 100. The published percentages are means of these marks. For certain special classes of phenomena—such as high-winds, frosts, and cold-waves—in which the indication only discriminates the occurrence and non-occurrence of a specific event, the formula for percentage of verification is

$$\frac{v}{n + o}$$

in which n is the whole number of times the event is predicted, v is the number of verifications, or of events coincident with predictions, and o is the number of unpredicted events.

It has been found for a large area in Europe, an area comparable

in size with one of the districts above mentioned, that, on the average, a given type of weather—*e. g.*, rain, threatening, fair, hot, cloudy, clear—can prevail simultaneously over only 85 per cent. of the area. If this law holds for the United States, we can hope for no better predictions while the existing system of districts is adhered to, for our percentage of verification is now approximately 85.

Mr. CURTIS described the method of verification adopted by the Deutsche Seewarte. It differs from that of the U. S. Signal Service in that the predictions are compared for verification with the observations at a single representative station in each district. Thus, for northwest Germany, the observations at Hamburg are employed. The limits for the prediction of stationary temperature are taken as $\pm$ 1° C., on the basis of an investigation by Hann who found that the "change in 24 hours at Hamburg, in two-thirds of all cases, averages less than two degrees C." Mr. CURTIS showed that, for verifications to be directly comparable with respect to skill in prediction, the limits for "stationary" must vary in different districts and at different seasons of they ear. Unless such variations are adopted, the verifications should exhibit a uniform geographical difference, and an annual period, if the method employed possesses any scientific accuracy. As any such change of definition would be impracticable, it would seem desirable to base the range allowed for "stationary" temperature entirely on physiological considerations, leaving the question of comparability for subsequent discussion.

In reply to a question by Mr. PAUL, Mr. ABBE said that the rules required that a prediction covering 24 hours should be verified by the maps compiled at the 8th and 16th hours, as well as by that compiled at the 24th. The desirablity of subdividing the geographic sections to which the weather predictions apply was discussed by Messrs. GILBERT, PAUL, and ABBE; and Mr. ABBE said that if any change was made, it would consist in the abandonment of specially defined districts and the substitution of individual States.

Mr. H. A. HAZEN remarked that if the prevailing weather in a district treated as a unit actually pertained, on an average, to but 85 per cent. of the district, then only omniscience could attain to a success in weather prediction measured by 85 per cent. of verification.

Mr. A. HALL made a communication on

VARIATIONS OF LATITUDE,

discussing the observations tabulated by Mr. Fergola, and reaching the conclusion that the evidence fails to show that latitudes are variable.

[The paper is published in the American Journal of Science, 3d series, Vol. XXIX, pp. 223–27.]

Mr. R. S. WOODWARD said that he had recently undertaken the discussion of the subject with somewhat fuller data than those used by Mr. HALL. Postulating that the pole was changing its position by motion at a uniform rate on the arc of a great circle, he computed the direction to be along the meridian 50° west of Greenwich, and the rate of motion about 2″ per century. His investigation was not yet completed, but he inclined to the opinion that actual change was indicated by the data used.

Other remarks were made by Messrs. FARQUHAR, BAKER, and PAUL.

266TH MEETING. MARCH 14, 1885.

The President in the Chair.

Fifty-four members and guests present.

The Chair announced the election to membership of Messrs. ROBERT PORTER HAINS and GEORGE MITCHELL WRIGHT.

The Chair read a letter from Mr. A. C. PEALE, announcing the death, on the 13th of March, of Mr. TITIAN RAMSAY PEALE, one of the founders of the Society. Mr. Peale accompanied Colonel Long in his explorations of the Rocky Mountains as naturalist, and was afterwards a member of the Wilkes' exploring expedition.

Mr. H. ALLEN HAZEN made a communication on

THUNDERSTORMS OF 1884.

This paper was a resumé of some of the investigations made by the Signal Office, looking to a detailed study of the origin, progress, and development of thunderstorms. Over 13,000 special reports were received and studied. An attempt to connect thunderstorm frequency with the phases of the moon showed a rather marked

increase during the time of new moon, thus corroborating the result previously obtained by Dr. Köppen.

A comparison of storm frequency with the period of solar rotation gave a marked maximum during the rotation. It was shown that taking the mean temperature over the whole storm region there was a close relation between the occurrence of high temperature and storm action, the former preceding the latter by about 24 hours.

Taking the mean of the meteorological elements on 20 days of many storms at many of the stations, it was found that a marked low-pressure area was present to the northwest of the storm region, there was also a high temperature, while the humidity and weather were normal. On 20 days of few storms the reverse was found true, namely, a relatively high pressure and low temperature, the humidity and weather being normal as before. These results were highly interesting as bearing upon the conditions favorable to thunderstorm action. The detailed study gives promise of large additions to our knowledge of these meteors.

In the ensuing discussing, Messrs. MUSSEY, RAY, ANTISELL, E. FARQUHAR, PAUL, GILBERT, ROBINSON, and HAZEN spoke of the topographic, geographic, and seasonal distribution of thunderstorms and of the relation of the precipitation to the electric phenomena. Mr. ANTISELL said that moisture is essential to their generation; they are a secondary effect of the influence of the sun, not a primary. Mr. E. FARQUHAR spoke of the concentration of electricity by diminution of aqueous surface, when cloud-particles coalesce and form raindrops.

Mr. S. M. BURNETT exhibited and explained

THE JAVAL AND SCHIÖTZ OPHTHALMOMETER.

Mr. A. B. JOHNSON began a communication on

THE DIFFICULTY IN DETERMINING THE DIRECTION OF SOUND.

267TH MEETING. MARCH 28, 1885.

The President in the Chair.

Forty-two members and guests present.

The Chair announced the election to membership of Messrs. GUSTAVE LOUIS RAVENÉ, THOMAS MAREAN CHATARD, HERBERT

Michael Wilson, Willard Adams Shumway, and Jefferson Franklin Moser.

Mr. A. B. Johnson then completed his communication on

THE DIFFICULTY IN DETERMINING THE DIRECTION OF SOUND,

illustrating his remarks by a model of the topophone. The following is an abstract of the entire paper.

Mr. Johnson said the hunter could not locate his game by the sound it made, unless the sound was frequently repeated; that the plainsmen could not locate each others' site by shouts, until they were frequently repeated; that a child calling its mother in a house could not tell which room she was in, or even the floor she was on, until her voice was heard several times; that it was hard to tell, from its noise alone, whether a street-car was going to the right or left, in approaching it at right angles; in fact that it was not easy to fix by the ear alone, the location of the source of any sound.

A dog aroused from sleep by the call of his unseen master frequently dashes in different directions before hitting the right one. Game startled by hearing a hunter's tread will as readily run into, as out of danger. Blind people, despite the highly developed condition of their remaining senses, do not appear to be more able to determine the source of sound, other things being equal, than seeing people. It seems to be a question whether people generally do not use sight, touch or smell, involuntarily in locating sound. Hence, when they are so placed that they must depend on hearing alone, and err unusually in doing so, they consider such instances as abnormal.

After referring to subjective errors in audition, which frequently arise, Mr. Johnson spoke of the peculiar class of errors in audition into which mariners are apt to fall, often resulting in disaster. The collision between the ocean steamers Edam and Lepanto, was referred to, in which the former was sunk, as the latter had erred an eighth of the compass circle in fixing her position by the sound of her fog signal, and thus ran into her. A lawsuit ensued, in which Judge Addison Brown, of the U. S. District Court of New York, decided against the plaintiff, holding "that an error of five points, in locating a vessel by the sound of her whistle in a fog, is not necessarily a fault, under the proved aberrations in the course of sound."

Mr. JOHNSON then read from Judge Brown's opinion, extracts from papers read before the Washington Philosophical Society by three of its former Presidents, Henry, Taylor, and Welling, and by himself, all as to the difficulty of determining the direction of sound, and he congratulated the Society that its conclusions had been adopted by the courts.

As it was evident that the unaided ear could not be relied upon to fix the direction from which sound came, Mr. JOHNSON said attention should be directed to giving the ear all possible assistance. That something of this kind could be done was proved, he thought, by Professor Morton's experimentation with the topophone. This instrument had been devised by Professor Mayer of the Stevens Institute of Technology. It consisted of an arrangement by which two Helmholtz resonators were connected on the deck of a steamer with rubber tubes running into the cabin and with bars and rods which could be moved from the cabin. The actuating principle of the device was the neutralization of the dynamic force of the full sound wave by the half sound wave, thus approximating silence, and thus indicating automatically, within ten degrees, or less than one point of the compass, the direction of the sound.

Mr. PAUL remarked that the bar connecting the resonators should be shorter than the wave length of the sound under observation, since otherwise deceptive results would be obtained with the two resonators in similar phases of different waves. Mr. TAYLOR questioned the utility of the instrument, though heartily applauding its ingenuity. The real difficulty in determining sound direction arises from the heterogeneity of the air in point of density and moisture, and especially from its indeterminate differences of movement, whereby diffractions and refractions are occasioned many times greater than those affecting light. The topophone, like the ear, is cognizant only of the final direction of the incident beam of sound, so to speak, and can tell us nothing of the direction of the source of sound. Mr. E. FARQUHAR remarked that the verdict of the ear in regard to direction is usually just; the conditions under which it errs are exceptional. He thought there was a rapid adjustment by motion of the head, from which the general direction is almost involuntarily ascertained. Mr. F. BAKER said that animals, such as, for example, the carnivora, make fewer mistakes than man, and this is probably due to their muscular control of the external ear. When the ears are pricked up in listening, special tensions may be given

to the *concha*. When man listens intently he adjusts the tensions of the *membrana tympani.*

The possible influence of wind velocity on the pitch of sound was discussed by Messrs. Paul, Taylor, and Gilbert, and other remarks were made by Messrs. King, Hall, Elliott, and H. Farquhar.

Mr. Washington Matthews began a communication on

MYTHOLOGICAL DRY PAINTINGS OF THE NAVAJOS,

which occupied the remainder of the evening. Its completion was deferred.

268th Meeting. April 11, 1885.

Vice-President Billings in the Chair.

Forty-six members and guests present.

Mr. Washington Matthews concluded his paper on

MYTHOLOGICAL DRY PAINTING OF THE NAVAJOS.

This paper described an art in use among the medicine men of the Navajo Nation, by which they represent various mythological conceptions on the sanded floor of the medicine lodge with dry pigments of five different colors. These dry paintings are from ten to twelve feet in diameter, and are quite intricate, containing from five to thirteen mythological figures of large size. About a dozen men labor from eight to ten hours in making them. When completed, they are after some ceremonies completely obliterated, and even the sand on which they are drawn is carried out of the lodge and thrown away. The existence of such an art is not generally known and the figures are not copied from any visible standard but are retained in the memories of the medicine men.

The paper was illustrated with seven water-color paintings—reproductions of the Navajo drawings. Four were pictures of the esoteric portion of a Navajo ceremony called dsilyidje-qaçal or "song in the mountains," and represented visions or revelations of the Indian prophet who instituted these ceremonies. The remaining three pictures were from a ceremony known as kledje-qaçal or "song of the night," and represented the revelations of another Navajo

prophet, he who, according to their mythology, instituted the latter ceremonies. The symbolism of the pictures was explained, and such portions of the myths as directly referred to the pictures were related.

The first picture showed the house of the great snakes. The second represented the gods of the domestic plants, with the principal domestic plants of the Indians, corn, bean, pumpkin, and tobacco, indicated by highly conventionalized figures. The third picture was of certain goddesses of great height, called the Bitsihi-nez or Long-bodies, which the prophet is said to have seen in a house made of dewdrops. The fourth drawing depicted the sacred arrows used in the dance, which the medicine men pretend to swallow. The lecturer explained the trick by which this imposture was carried out. The fifth picture represented the peculiar myth of the tsis-naole or whirling sticks. It represents two logs placed in the center of a lake so as to form a cross. Eight divine beings sat on these logs, which were kept constantly whirling by other gods who poked the logs with plumed staves. There were twelve apotheosized human figures in the picture. The sixth picture showed the kledje qaçal as it took place in the abodes of the gods when the Navajo prophet first saw it, and is a fair representation of the dance as it is performed among the Navajos to-day. The seventh painting represented a portion of the dance among the gods, at a time when a spell had been cast upon them by the angry Coyote-god.

The figures in the east of the pictures are painted in white, those in the south blue, those in the west yellow, those in the north black. This is the usual order of Navajo color symbolism; but sometimes the white is assigned to the north and the black to the east; instances were given where this interchange took place.

The gods in many cases are shown standing on rafts made of sunbeams, such rafts being favorite vessels of the gods when they make their aerial journeys. The gods are depicted with round heads, the goddesses with quadrangular heads. In the dances, the actors wear masks of corresponding shapes to indicate males and females.

Seven of the pictures were surrounded with symbols of the rainbow deity, which with the Navajos, as with the Greeks, is a goddess.

The sanded floor on which the pictures are drawn is slightly sprinkled with charcoal; this is to convey the idea of a surface of clouds, for it is said that in the houses of the gods these pictures were drawn on sheets of clouds.

The speaker closed by referring to the transitory nature of the pictures, and showing how it might easily have happened that no knowledge of them would ever transpire.

Mr. GILBERT THOMPSON described pictures on the walls of a shallow cave near San Antonio Spring, New Mexico, and exhibited copies of the same. The outlines of the pictures are etched on the rock, and several different colors are employed, both in the etched grooves and on the plane surface of the rock. Mr. MATTHEWS explained the relations of these drawings to Navajo myths and ceremonies.

In response to questions by Messrs. BILLINGS, M. BAKER, PAUL, and MALLERY, Mr. MATTHEWS said that individual drawings are not repeated on the same occasion. The ceremonial dances, most of which take place only during the season when the snakes hibernate, are executed for the benefit of invalids, or for the gratification of individuals who by conventional fiction are regarded as ill. They are paid for, and they are very expensive luxuries, the gross bill of expenses sometimes amounting to the value of $300. The patient or his friends select the particular dance to be performed. After the completion of the picture, the patient enters the lodge, and is seated upon the east figure, while a litany is chanted. Sand from one of the painted figures is then applied to his body, sand from the arm being applied to his arm, &c.

Mr. PAUL described a similar art of dry-painting, practiced by the Japanese, but for amusement only. Bold designs of great variety are executed skillfully and rapidly in public places, for which the artist receives compensation from the by-standers. Wealthy Japanese also employ persons to dance for them, and, for that matter, to fish for them; but the motive appears to be pleasure, and not religion, or health.

Other remarks were made by Mr. JENKINS.

Mr. W. C. WINLOCK made a communication on

COMETS II AND III, 1884,

illustrating his subject by models exhibiting each cometary orbit in its proper relation to the earth's orbit, and also by plane diagrams and sketches of the comets.

Mr. Ravené spoke of the perturbations of Barnard's comet occasioned by the attraction of Jupiter, and thought it might have been brought into the solar system by that attraction.

Other remarks were made by Mr. Paul.

Mr. H. M. Paul commenced a communication on

PROBLEMS CONNECTED WITH THE PHYSICS OF THE EARTH'S CRUST.

Its completion and discussion were deferred to a future meeting.

269th Meeting. April 25, 1885.

The President in the Chair.

Fifty-three members and guests present.

Announcement was made of the election to membership of Mr. Charles Frederic Adams.

Mr. Frank Baker made a communication on

MODERN IDEAS OF BRAIN MECHANISM.

Remarks were made by Messrs. E. Farquhar, H. Farquhar, M. Baker, and Doolittle.

Mr. L. F. Ward began a communication on

THE FLORA OF THE LARAMIE GROUP,

the completion of which was deferred for lack of time.

270th Meeting. May 9, 1885.

The President in the Chair.

Forty-four members and guests present.

Announcement was made of the election to membership of Mr. Walter Harvey Weed.

Mr. Lester F. Ward completed the reading of his communication on

THE FLORA OF THE LARAMIE GROUP.

[It will appear in the Sixth Annual Report of the U. S. Geological

Survey, as a portion of the author's "Synopsis of the Flora of the Laramie Group."]

Remarks followed by Messrs. GILBERT, ELLIOTT, and WHITE.

Mr. T. C. MENDENHALL made a communication on

THE MEASUREMENT OF TEMPERATURE AT DISTANT POINTS.

Remarks were made by Mr. ELLIOTT.

Mr. GUSTAVE RAVENÉ gave an abstract of a communication prepared on

THE ASTEROIDS.

Remarks were made by Messrs. TAYLOR, H. FARQUHAR, and ELLIOTT.

271ST MEETING. MAY 23, 1885

The President in the Chair.

Forty-one members and guests present.

The Chair announced that only one more meeting would be held before the summer vacation.

Mr. A. GRAHAM BELL made a communication on

THE MECHANISM OF "CLICKS" AND "CLUCKS."

Remarks were made by Messrs. M. BAKER, GILBERT, and ROBINSON.

Mr. H. M. PAUL completed his communication on

THE CONDITION OF THE EARTH'S INTERIOR.

Mr. W. B. TAYLOR made a communication on

THE CRUMPLING OF THE EARTH'S CRUST,

in which, referring to the plausible hypothesis of contraction by cooling—which had been so largely accepted, he contended that the amount of cooling and contraction since the formation of a consistent crust had been much less than even the opponents of that hypothesis had conceded; while the maximum amounts estimated by its adherents would still be wholly inadequate to represent the

actual measure of compression indicated by the average degrees of plication of the stratified rocks. Supposing these to represent a a reduction from the original circumference of the crust of one-eleventh, this would involve a former excess of volume of about one-third.

The speaker then gave an historical sketch of the growing conviction among physicists that from the tidal retardation of the earth's rotation, the length of the day must have been much shorter in remote geological eras than at present—and consequently the oblateness of the terrestrial ellipsoid considerably greater. Estimating that a day of six hours would give an equatorial enlargement of about one-tenth (without taking any account of *volumetric* change by reduction of temperature), he thought this *morphologic* change an adequate explanation of the observed crumpling of the earth's crust; and claimed that the cause assigned is both a true and a sufficient one. This larger oblateness would imply an equatorial bulge 396 miles greater in radius than the present; and a corresponding depression of the poles 658 miles below their present levels. As in a general way confirmatory of this hypothesis, Guyot's statement was quoted that "On the whole, the reliefs begin with the vast, low plains around the polar circle, and go on increasing from the shores of the Arctic ocean toward the tropical regions;" and that "the ocean basins become less deep toward the North pole—just as the lands become lower toward the same region."

[This paper is printed in full in the American Journal of Science, 3d Series, Vol. XXX, pp. 249–266.]

272ND MEETING. JUNE 6, 1885.

The President in the Chair.

Twenty-eight members present.

Mr. J. P. IDDINGS made a communication on

THE COLUMNAR STRUCTURE IN THE DIABASE OF ORANGE MOUNTAIN, N. J.

[Abstract.]

The paper describes the occurrence and structure of the "trap" rock in the neighborhood of Orange, with special reference to the arrangement of the columns in John O'Rourke's quarry and in the

Undercliff quarry in Llewellyn Park. The chief interest centers in the groups of radiating columns which form the upper portion of the exposures, the lower portion being divided into vertical columns or blocks of larger size. That these two portions of the lava sheet belong to one and the same mass is shown, not only by the continuity of the rock of the upper and lower parts, but also by the mutual accommodation of the different sets of columns, which taper off and curve in one direction along lines of oblique junction; and by the fact that the positions of the columns are not what they should be along the supposed lines of contact.

The columnar structure in volcanic lavas is unquestionably a cracking produced by shrinkage upon further cooling, after the mass has consolidated into rock.

Considering the origin and progress of a crack produced by the shrinkage of a homogeneous mass, we see that, starting with a plane surface over which forces producing contraction are acting uniformly, the contraction produced on the surface of the mass in a given time will be greater than that produced at some depth within the mass, and that it will decrease gradually from the surface inward. As the contraction progresses, the limit of tension in the direction of the surface will be reached before that in the direction of depth, causing a rapture across the direction of the surface, and as the limit of tension for the layer next to the surface is reached it will rupture in the same direction as the surface layer did, and so on. The direction of the crack is at right angles to that of greatest contraction, or normal to the line of maximum strain. Moreover the condition of the mass at the moment the limit of tension along the surface is reached may be graphically represented as in figure 1, the contraction being a maximum in the

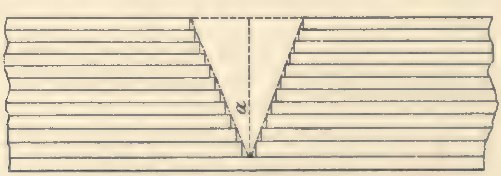

Fig. 1.

top layer and diminishing successively in each layer beneath to that with the initial expansion. The distance of this layer from the surface being taken as unity, the maximum contraction at the

moment of rupture will be equal to twice the tangent of a. In other words $2 \tan a$ represents the limit of tension, and will be constant for any given substance.

As the conductivity of a cooling body is not directly proportional to the degree of radiation from its surface, the difference between the contraction of successive layers of a rapidly cooling mass will be greater than between those of one cooling less rapidly, and what may be styled the angle of contraction will be greater in the former case than in the latter. And if we assume a certain rate of cooling to have caused a single rupture in a given extent of mass (represented in fig. 1), then a greater rate of cooling, which would produce in the same extent of mass a contraction represented by a greater angle, β, will cause as many ruptures as the ratio, $\frac{\tan \beta}{\tan a}$.

If the forces producing contraction are unequally distributed over the surface $a\ b$, figure 2, being a maximum at c, the maximum

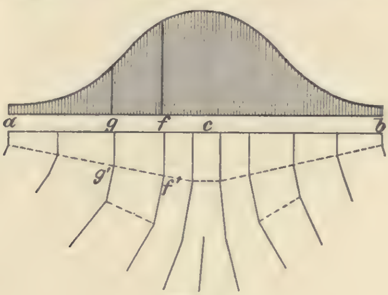

Fig. 2.

strain at the beginning will be in the direction of the surface, and the cracks will start normal to it; but their progress inward will no longer be uniform. At the end of a given time the limit of tension reached by a greater force, at f, will be farther from the surface than that reached by a less force, at g, and the line of maximum strain in this portion of the mass will be $g'\ f'$, to which the crack of parting will be normal. At the end of another given time the direction of the crack will be again changed, and the same action taking place in the other parts of the mass will result in a system of diverging cracks.

So far we have considered the shrinkage in one direction in one plane only, that is, parallel to the cooling surface in a plane at right angles to it. But a homogeneous mass contracts equally in all

directions, and the contractile force which produces cracks at certain distances in a given mass will exert itself equally in all directions over a surface uniformly subjected to the cooling forces, and will, at the instant of fracture, act towards centres, whose distance apart is dependent on the rate of cooling. If the mass is perfectly homogeneous the centres of contraction will be disposed over the surface with the greatest uniformity possible, that is, they will be equidistant throughout, and the resultant fractures will be in a system of hexagons. If from any irregularity in the composition or petrographic structure of the rock the contractile force acts unequally in different directions, the form of the polygons will be less regular.

The mutual influence of the forces producing different columns as they approach each other is readily understood from the foregoing. Take the case of two columns, S, S', approaching one another (Fig. 3,) and suppose the progress of the maximum strain to have reached

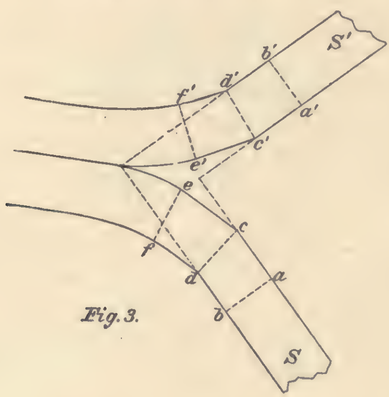

Fig. 3.

$a\ b$, $a'\ b'$, the forces producing contraction acting through a and a' will meet and react on each other before those acting through b and b', so that the points of maximum strain at any given time will have advanced farther along the lines through a and a' than through b and b'. The lines of greatest strain will then be $c\ d$ and $c'\ d'$, and the cracks normal to them will take the directions $c\ e$ and $c'\ e'$. This will continue till they become parallel.

If there were but two columns forming at equal rates they would curve symmetrically and continue in parallel directions and of constant width, but if one column progresses more rapidly than the

other they will no longer curve to the same extent, and the slower one will curve more than the faster one.

Now, instead of two single columns, there are always two groups approaching one another, and these prevent the continuation of the columns beyond the bend, pinching them out and causing them to taper off as already observed in the quarry described.

The difference in the systems of cracks of the lower and upper portion of this lava sheet may then be accounted for by a difference in the rate of cooling from the lower and upper surfaces, the more frequent fractures arising from the more rapid cooling, and the two systems proceeding from their initial planes until they blend in one another within the mass.

If for any reason the cooling from one surface should take place irregularly and from any point proceed more rapidly than from others, it is evident that there would result a set of columns diverging from this point as a focus.

Besides the columnar fracturing, a division of the mass by transverse cracks, especially near the top of the lava sheet, is more or less noticeable.

There remains to be considered the contraction exerted in all other directions through the mass. Since the uniform contraction of a homogeneous body acts equally in every direction through it, its effect corresponds to the equal shortening of the radii of a sphere of such a body. If through any resistance cracking or parting occurs it will take the form of concentric spherical shells.

If for any reason the resistance to the contracting force in a particular direction be counteracted by some other force acting in the same direction the parting will no longer be spherical, but ellipsoidal, as will be seen from Figure 4, where *a b c* represents a section

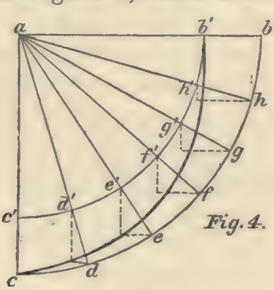

Fig. 4.

through the sphere along the radii of which contraction takes place. A uniform resistance in the direction of the radii represented by

$c\ c'$, $d\ d'$, etc., will produce a parting parallel to the arc of the circle $c'\ d'\ e'$, etc. If, however, the resistance in a direction parallel to $a\ c$ be neutralized by some force, the resistance along the different radii will be diminished by the amount of the vertical component in each case, and the resulting fracture will be parallel to the ellipse $c\ b'$. The relative tendency to fracture also is represented by the area $b\ c\ b'$.

Such a parting is actually present in the large columns in John O'Rourke's quarry, the major axis of the ellipsoid being vertical, as it should be if the weight of the superincumbent mass counteracted any resistance to contraction in a vertical direction.

The wavy form of the columns, large and small, suggests irregularities in the mass which disturbed the uniform advance of the lines of maximum strain and caused them to deviate from parallelism.

The superficial banding of the large vertical columns by nearly horizontal notches or grooves, resembling layers of bricks or rude chiseling, appears to be simply a modification of the plane of the crack.

The paper closes with a description of the microscopical character of the Orange rock, which from its identity with many recent basalt flows leads the writer to the conclusion that it should be classed as a coarse-grained basalt or dolerite, as Prof. E. S. Dana has called the similar rocks in the Connecticut valley.

The occurrence of the rock in question as a surface flow is rendered highly probable by its glassy nature, and the disposition of the columns, which resembles that of many lava sheets in western America, as well as of those in central France. G. Poulett Scrope, in his work on "Volcanos" (2d edition, London, 1872), discusses the question of the origin and nature of columnar structure in lavas and other substances, and by a somewhat different course of reasoning arrives at essentially the same conclusions as those reached in the present paper.

Mr. W. J. McGee then made a communication on

THE TERRACES OF THE POTOMAC VALLEY.

Remarks were made by Messrs. Ward, Toner, Robinson, and Bates.

273D MEETING OCTOBER 10, 1885.

The President in the Chair.

Thirty-nine members and guests present.

Announcement was made of the death since the last meeting of two members of the Society, FRANKLIN BENJAMIN HOUGH and WASHINGTON CARUTHERS KERR.

Messrs. J. S. BILLINGS and WASHINGTON MATTHEWS made a joint communication on

ANTHROPOMETRIC AND REACTION-TIME APPARATUS.

They exhibited a set of the anthropometric apparatus, devised chiefly by Mr. Francis Galton, and recently employed in the anthropometric laboratory of the London Health Exhibition. The apparatus test acuteness of hearing, strength of vision, color discrimination, the estimation of the aliquot parts of a line, the estimation of a right angle, the rapidity of arm movement in striking a blow, the strength of certain muscles, the weight, the height (sitting and standing), span of arms, and chest capacity. Their peculiar characteristic is their simplicity, which permits of their use by the person measured, with a minimum of instruction and supervision. There was also exhibited a device by Mr. James McKeen Cattell, for the determination of the time occupied by various sensations, mental processes and muscular actions.

There followed an informal discussion by Messrs. H. A. HAZEN, E. FARQUHAR, HALL, H. FARQUHAR, HARKNESS, MUSSEY, WOODWARD and MASON.

274TH MEETING. OCTOBER 24, 1885.

The President in the Chair.

Fifty-three members and guests present.

Mr. H. ALLEN HAZEN made a communication on

THE CONDENSING HYGROMETER AND SLING PSYCHROMETER.

[Abstract.]

By way of introduction the results of a few experiments were given, tending to show the best interval that can be obtained in the

graduation of degrees upon a thermometer scale. These were made with a common vernier of a mercurial barometer. A mark on the vernier was placed at each tenth (by estimation) of intervals of .05″, .10″, .15″, .20″, .30″, and .40″, marked on the limb, and the vernier read for each estimation. Over 1300 readings were made, and these showed little difference in the splitting to tenths for the last 5 intervals above, but .05″ seemed too small for accurate work.

Results and methods of observing the Alluard form of Regnault's condensing hygrometer were given. It was shown that if the thermometer immersed in the liquid is placed quite near the plate where dew is to appear, there is little or no danger of the air as it passess into the liquid harmfully affecting the thermometer. The complaint of some that the dew is deposited in the air, having a temperature frequently 40 or 50 degrees above the liquid, and hence that the thermometer can hardly give a correct dew-point temperature was shown to have little weight, since the results, with a slight difference between the air and dew-point, at which time the effect would be small, are nearly identical with those where the difference is large, and the effect would be large. It was shown that the great difficulty in nearly all psychrometric work up to the present time has been the disregard of a sufficient ventilation of the wet-bulb thermometer. The sling psychrometer, with a few precautions in its use, furnishes results entirely satisfactory.

The comparisons so far made between the two instruments have shown a remarkable uniformity under all conditions of moisture and temperature, and have left little to be determined in order to make either apparatus one of precision. A probable effect of compression of ice on the wet bulb at temperatures of 0° F., and below, was shown to exist, though this may be due to the lack of conduction on the part of the ice for the residuum of heat in the bulb. The question of the effect of height above sea on the indications of the above instruments was touched upon, and it was shown that the effect was small and only to be detected by the most refined observations. Also, that until we have some law for reducing humidity results to sea level, the propriety of introducing such effect into tables is questionable.

Mr. T. C. Mendenhall exhibited a new volt-meter devised by Sir William Thompson. The principal difficulty encountered by earlier instruments of this class has arisen from the inconstancy of the magnetic force of the terrestrial field. By this instrument ter-

restrial magnetism is eliminated. The force produced by the current is opposed by a weight and is thus measured in terms of gravity.

Mr. MENDENHALL also renewed the discussion of the preceding evening on reaction time, reciting the methods and results of his own experiments in 1871.

Remarks on the volt-meter were made by Mr. ELLIOTT, on reaction time by Messrs. PAUL, MASON, and MATTHEWS.

Mr. WILLIAM HARKNESS made a communication on

FLEXURES OF TRANSIT INSTRUMENTS,

pointing out that the flexures induced by the weight of a transit, in positions other than vertical, are not eliminated by reversing the instrument, and developing equations for the discussion of the errors so far as they can be determined by the aid of collimators.

275TH MEETING. NOVEMBER 7, 1885.

The President in the Chair.

Sixty-nine members present.

Mr. F. W. CLARKE made a communication on

AN ATTEMPT AT A THEORY OF ODOR,

in which he accounted for the lack of knowledge as to the conditions of action of this sense by the difficulty of dissociating it from taste; and, while disclaiming any thought of attempting a physiological explanation of the sense, proposed the following as the essential objective conditions:

1. To be odorous, a substance must be volatile, so that it may come into contact with the mucous tissue of the nose, and

2. It must be chemically unstable, so that it may undergo chemical changes in contact with that tissue.

Mr. Clarke gave some confirmatory instances, from the compounds of hydrogen with sulphur, selenium, and tellurium, and from the $C_n H_{2n} O_2$ group of acids (formic, acetic, etc.)

Mr. ANTISELL called attention to the connection between a low boiling-point and simplicity of chemical constitution, and to the associated fact that organic substances containing a large number

of equivalents of carbon are inodorous. He ascribed the smell of prussic acid to *arrière-gout* rather than true odor.

Mr. Billings showed that from the peculiarly exposed condition of the olfactory nerve-terminal, it was subject to irritations that must be distinguished from odors properly speaking.

Mr. Clarke then made a communication on

THE FLOOD ROCK EXPLOSION,

in which he described the arrangements for observing earth tremor at stations near New York city, and particularly that at Ward's Island, occupied by Mr. Mendenhall and himself. The tremor was felt at that station a full second before any disturbance was seen in the surface of the water above Flood Rock.

Mr. C. F. Marvin, in a communication on the same subject, described the form of seismoscope used, in which a small agitation closed an electric circuit, and sounded an alarm.

Mr. Paul followed with a communication on the same subject.

Mr. Clarke quoted some results of observations made at Gen. Abbot's stations, giving a mean velocity for the tremor from Flood Rock to Pearsall's of 2.6, and to Patchogue of $2\frac{1}{4}$ miles per second, and thus indicating a retarded rate of transmission.

Mr. Robinson suggested that the blasts at the new water-works reservoir would afford a good opportunity for measuring the velocity of earth tremors.

Mr. H. Farquhar spoke of the sounds coincident with the flight of meteors reported by some observers, as indicating the need of caution in accepting observations of sound in this connection.

Mr. Robinson had distinctly heard two sounds after blasts at the water-works; one immediately following the tremor, through the earth, and a later one through the air.

Mr. Chatard had made a similar observation in connection with mining blasts.

Mr. Dutton said that his impression, from eruptions of Hawaiian volcanoes, had been otherwise, and that the general testimony with regard to earthquakes is that the sound precedes the shock.

276TH MEETING. NOVEMBER 21, 1885.

The President in the Chair.

Fifty-seven members present.

Announcement was made of the election to membership of Mr. THOMAS HAMPSON.

The Chair communicated to the members an invitation from the Chemical Society to attend its meeting on December 10th and listen to the address of its retiring president, Prof. F. W. CLARKE.

Mr. G. BROWN GOODE and Mr. C. V. RILEY made communications on

THE SYSTEMATIC CARE OF PAMPHLETS,

exhibiting the appliances employed by them and illustrating their methods.

Mr. GOODE furnishes each pamphlet with a firm, durable cover, by which it is protected from injury, and at the same time kept separate for convenient use and classification. Photographs, drawings, newspaper clippings, etc., are preserved in the same manner. [A full description of his appliances may be found in Science, vol. VI, p. 337.]

Mr. RILEY employs inexpensive, flexible covers, occupying less space, and stores them in the "institute pamphlet case."

Mr. B. PICKMANN MANN, being invited to participate in the discussion, exhibited his method of binding pamphlets together, a method in which four holes are punched at standard intervals in each pamphlet and corresponding holes in flexible hinges to stiff covers, so that a convenient volume can be made up by merely inserting and tying two cords, and any desired rearrangement or insertions can be made, the holes for binding always corresponding. [See Science, vol. VI, p. 407, and Library Journal, vol. VIII, p. 6.]

Mr. BILLINGS described the tin boxes used in the storage of the immense file of pamphlets in the library of the Army Medical Museum. Mr. WARD and Mr. TONER spoke in approval of the substantial covers adopted by Mr. Goode. Messrs. GILBERT, TONER and HARKNESS opposed the binding of several pamphlets together, believing that such combination interferes with their use and ready classification and reclassification.

Other remarks were made by Messrs. DALL, MUSSEY, and DOOLITTLE.

Mr. J. S. BILLINGS made a communication on

GERM CULTURES,

exhibiting specimens of cultures of chromogenetic and pathogenetic micro-organisms, to illustrate the improvements in methods of investigating these organisms which have been made of late years.

Attention was called to the value of culture upon semi-solid media such as peptonized gelatin, agar agar, coagulated blood serum, etc., as a means of differentiating micro-organisms, and of obtaining pure cultures to be used for experimental purposes; and the applications of the method to testing the efficacy of disinfectants, to water examinations, &c., were pointed out.

277TH MEETING. DECEMBER 5, 1885.

By courtesy of the officers of the Columbian University, the meeting was held in the law lecture room of the University building. Members of the Anthropological, Biological, and Chemical Societies and their friends were present by invitation.

Vice-President BILLINGS occupied the Chair.

Present, one hundred and sixty members and guests.

The President, Mr. ASAPH HALL, read his annual address, taking for his subject:

AMERICAN SCIENTIFIC SOCIETIES.

[Printed in full on pp. XXXIII–XLVII.]

A resolution of thanks was moved and unanimously passed.

278TH MEETING. DECEMBER 19, 1885.

THE FIFTEENTH ANNUAL MEETING.

The President in the chair.

Thirty-eight members present.

The minutes of the 260th, 276th, and 277th meetings were read and approved.

The Chair announced the election to membership of Mr. Jacob Lawson Wortman.

The report of the Secretaries was read and accepted.

The report of the Treasurer was read, received, and referred to an auditing committee, consisting of Messrs. J. M. Toner, O. T. Mason, and T. C. Mendenhall.

On motion of Mr. Harkness, the thanks of the Society were tendered to the Treasurer and Secretaries for the efficient performance of the duties of their offices.

Officers were then elected for the year 1886. (The list is printed on page xv.)

The rough minutes of the meeting were read, and the Society adjourned.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

MATHEMATICAL SECTION.

STANDING RULES

OF THE

MATHEMATICAL SECTION.

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1. The object of this Section is the consideration and discussion of papers relating to pure or applied mathematics.

2. The special officers of the section shall be a Chairman and a Secretary, who shall be elected at the first meeting of the Section in each year, and discharge the duties usually attaching to those offices.

3. To bring a paper regularly before the Section it must be submitted to the Standing Committee on Communications for the stated meetings of the Society, with the statement that it is for the Mathematical Section.

4. Meetings shall be called by the Standing Committee on Communications whenever the extent or importance of the papers submitted and approved appear to justify it.

5. All members of the Philosophical Society who wish to do so may take part in the meetings of this Section.

6. To every member who shall have notified the Secretary of the General Committee of his desire to receive them announcements of the meetings of the Section shall be sent by mail.

7. The Section shall have power to adopt such rules of procedure as it may find expedient.

OFFICERS

OF THE

MATHEMATICAL SECTION FOR 1885.

Chairman, G. W. HILL. *Secretary*, MARCUS BAKER.

LIST OF MEMBERS WHO RECEIVE ANNOUNCEMENT OF THE MEETINGS.

ABBE, C.
AVERY, R. S.
BAKER, M.
BATES, H. H.
BILLINGS, J. S.
BURGESS, E. S.
CHRISTIE, A. S.
COFFIN, J. H. C.
CURTIS, G. E.
DELAND, T. L.
DOOLITTLE, M. H.
EASTMAN, J. R.
EIMBECK, W.
ELLIOTT, E. B.
FARQUHAR, H.
FLINT, A. S.
GILBERT, G. K.
GORE, J. H.
GREEN, B. R.
HALL, A.
HALL, A. Jr.
HARKNESS, W.
HAZEN, H. A.
HILGARD, J. E.
HILL, G. W.
HODGKINS, H. L.
KING, A. F. A.
KUMMELL, C. H.
LEFAVOUR, E. B.
MCGEE, W. J.
NEWCOMB, S.
PAUL, H. M.
RAVENÉ, G. L.
RITTER, W. F. M'K.
ROBINSON, T.
SMILEY, C. W.
STONE, O.
TAYLOR, W. B.
UPTON, W. W.
WINLOCK, W. C.
WOODWARD, R. S.

BULLETIN

OF THE

MATHEMATICAL SECTION.

16TH MEETING. JANUARY 7, 1885.

The Chairman, Prof. ASAPH HALL, presided.

Nineteen members and guests present.

Election of officers of the Section for the year 1885 was then held and resulted in the selection of Mr. G. W. HILL as Chairman and Mr. MARCUS BAKER as Secretary.

Mr. E. B. ELLIOTT then presented a communication entitled

EXAMPLE ILLUSTRATING THE USE OF A CERTAIN SYMBOL IN THE CALCULUS OF AFFECTED QUANTITY.

The example selected was a demonstration of the Pythagorean Theorem by the aid of a new symbol.

Mr. MARCUS BAKER presented a communication entitled

A COLLECTION OF FORMULÆ FOR THE AREA OF A PLANE TRIANGLE,

which elicited some criticism of details and of notation.

[This paper is published in full in the *Annals of Mathematics*, vol. 1, No. 6, and vol. 2, No. 1.]

Mr. W. C. WINLOCK (by permission of Rear Admiral S. R. Franklin, Superintendent U. S. Naval Observatory) presented a communication on

PHYSICAL OBSERVATIONS OF WOLF'S COMET (1884 III).

[Abstract.]

The first observation of Wolf's comet that I obtained was with the transit circle of the U. S. Naval Observatory on September 24,

1884. The aperture of the instrument is 8 inches and the magnifying power employed 186.

On October 13, using the 9-inch equatorial and a power of 132, "the comet seemed to be a circular nebulous mass with quite a well marked central condensation. The nucleus was not sharply defined, but blended gradually into the fainter light surrounding it."

Nov. 8. Transit circle. Nucleus quite well defined. Faint but not difficult to observe.

Nov. 12. Transit circle. Nucleus elongated in the *preceding following* direction and apparently composed of a number of bright points. Faint and difficult to observe. The nebulous envelope seems to extend farther on the upper or south side than on the lower side. Seeing, very poor.

Nov. 13 with the 9-inch equatorial and the same power as before a sketch (omitted here) was made. The remarks I give substantially as in my note-book: Watched the comet carefully for about an hour (seeing not very good,—a little fog hanging over the river). It is a very slightly oval nebulous object. The central part is a little condensed; the nucleus proper more so. Filar micrometer measures give for the extent of the outer nebula, measured in the direction of a circle of declination, 1′ 52″, and for the inner disc 18″. The nucleus is perhaps extended a little in the *preceding following* direction, but I looked in vain for any indication of the beaded appearance which I thought I saw last night with the transit circle. I might add that using the distance of the comet given in Krüger's ephemeris these measures would represent distances of 47,000 and 7,500 miles, respectively. No tail.

Nov. 20. Transit circle. Nucleus stellar, 10th magnitude.

On November 22 another observation was obtained with the 9-inch equatorial, magnifying power 132 as before. Micrometer measures of the outer nebula and the inner disc gave 1′ 30″ and 16″, respectively, differing but little from the measures of the 13th. Seeing, fair. The following note was made: I divide the comet into three parts, the outer nebula, the inner envelope (or coma I presume it might be called), and the nucleus proper. It is almost impossible to assign a definite limit to this outer envelope, wavering and flickering like a mass of smoke, but the micrometer measures will fix it roughly. The inner envelope blends into the outer without any sharp division, though there is sufficient difference in brightness to attempt a measurement. The inner envelope condenses in turn into the brighter nucleus.

Nov. 24. Transit circle. Nucleus sharp and stellar and about 10th magnitude.

Dec. 2. Transit circle. Seeing poor. Extremely faint. Like a 12.5 magnitude star, surrounded by a large but faint nebula.

Dec. 8. 9-inch equatorial and power 132. The diameter (in declination) of the outer envelope, from a micrometer measurement, was 2′ 21″, the seeing being noted a little better than on Nov. 22. Occasionally I think I see the inner condensed disc, but am not sure of it; also think that at times there is an indication of a more or less rounded outline to the head on the *south preceding* side, but it is unstable. Cannot be sure of anything like a tail, and indeed any definite form other than an irregular circle is, after all, largely a matter of imagination.

The communication gave rise to some comment and discussion on the difficulties encountered in making satisfactory observations of faint comets and also on the resisting medium in space.

Mr. TAYLOR called the attention of the Section to

A SLIGHT MODIFICATION OF THE NEWTONIAN FORMULA OF GRAVITATION

with which he had been struck in reading Mr. Bates' paper on "The Physical Basis of Phenomena" recently read before the General Meeting. (*See* vol. vii, p. 51.)

[Abstract.]

There is a widespread fallacy—particularly displayed by those kinematists who fancy they have an exceptional insight into the "mechanism of gravitation," that this influence is simply a radiant emanation, necessarily observing the geometry of radial space relations, having as such emanation the same total energy on all concentric spheres whatever their radii, as in the case of luminous radiation for example. Of course every well instructed astronomer and physicist knows that this is not so. In truth "the inverse square" is not *geometrical*—not a square at all, having no relation whatever to surface,—but simply an *algebraical* second power, very much like the familiar "velocity squared" ($m\ v \times v$, or momentum multiplied by velocity), which forms the measure of all kinetic energy and which no one supposes to represent a square.

Our late colleague, General Alvord, in confutation of this not unusual misconception, made a communication to the Society some two or three years ago (as those present doubtless remember) in which he showed that as gravitation was known to act equally on every particle of matter (*i. e.* proportionally to the mass) and as solid homogeneous spheres subtending any given conical angle from a center of reference possess volumes (or masses,—d being constant) directly proportional to the cubes of the conical altitudes or radii of distance, it follows—if gravity were a radial emanation—its effect must obey the law of inverse *cubes* of distance, contrary to the facts of observation.

The fallacy here criticised springs evidently from the too common tendency to regard gravitation simply as a central force or as a single influence radial in direction, whereas it is always a duplex and reciprocal action; and however insignificant one of the terminal elements its presence and measure of distance cannot be neglected without completely nullifying all action. Thus m and m' being two masses at any given distance apart, the action in the direction and through the distance $m' m$, is as real and positive as that in the direction and through the distance $m m'$. In other words, it would seem that the *mutuality* of the re-action necessarily involved with it the idea of *reciprocity* of the distance relation. Thus, adopting the suggestion of Mr. Bates, if we write the formula of the effect as $(m \div d) \times (m' \div d)$, we have this reciprocity distinctly brought out, and obtain at once the Newtonian formula. The speaker wished to learn from those more conversant than himself with mathematical literature whether the suggested modification is new, and also whether any mathematical objection appears to its form.

Mr. HILL remarked that one fault of the notation proposed appeared to be its want of *generality*, as it is evidently inapplicable to any other force having a higher or different exponent of the space function.

Mr. DOOLITTLE observed that, admitting the "reciprocity of the distance relation," he yet failed to perceive how this function could appear in the formula as a *product*. Why should we write the distance twice taken—as d multiplied by d rather than as d plus d?

Further remarks were made by Messrs. ELLIOTT, BATES, and ROBINSON.

17TH MEETING. FEBRUARY 10, 1885.

The Chairman, Mr. G. W. HILL, presided.

Present, eleven members and one guest.

In the absence of the Secretary the reading of the minutes of the last meeting was deferred, and Mr. R. S. WOODWARD designated as Secretary *pro tem.*

Mr. KUMMELL made a communication on

AN ARTIFICE SOMETIMES USEFUL FOR THE ADJUSTMENT OF CONDITIONED OBSERVATIONS.

[Abstract.]

The general process consists in multiplying equations of condition by such factors as will extinguish side-coefficients in the normal equations for correlates. This was shown to be possible in an infinite number of ways. One such way leading to linear equations for the multipliers was shown to require for the extinguishment of *all* the side-coefficients the solution of the normals, *i. e.*, the very work which was to be evaded. The method would, however, be advantageous for the partial extinguishment of large side-coefficients, and the normal equations could thus be solved with advantage by Gauss' indirect method. A useful symmetrical rule was given for extinguishing the side-coefficients for a *pair* of conditions. Illustrations of this rule in the adjustment of simple geometrical figures were given, beginning with a simple figure of two triangles and extending to a complete pentagon.

Mr. HILL remarked that Jacobi had proposed a similar method for removing side-coefficients. Further remarks were made by Mr. WOODWARD.

The next communication was by Mr. GUSTAVE L. RAVENÉ on

THE THEORY OF MERCURY.

[Abstract.]

The method here used of computing the secular variation of the elements of an orbit is due to Gauss.

The notation employed for the *disturbed* body is

π = longitude of perihelion;
θ = longitude of the ascending node;
i = inclination to the ecliptic;
a = mean distance from the sun;
n = mean annual motion;
e = eccentricity;
φ = eccentric angle = arc sin e;
r = radius vector;
f = true anomaly;
ε = eccentric anomaly;
m = mass; and the same symbols with accents, π', θ', i', etc, represent corresponding quantities for the *disturbing* body.

From the definition of the secular perturbations, according to Gauss, the perturbing function may be expressed by

$$V = \frac{m'}{4\pi^2}\int_0^{2\pi}\int_0^{2\pi}\frac{1 - e\cos\varepsilon}{\rho}\,d\varepsilon\,d\varepsilon'$$

in which ρ is expressed by

$$\rho^2 = a'^2 + r^2 - 2a'\,r\cos(a', r).$$

We also have

$$r^2 = a^2(1 - e\cos\varepsilon)^2$$

$$a'r\cos(a', r) = a'\cos\varepsilon'\left\{\cos\pi\,(a\cos\varepsilon - ae) - a\cos\varphi\sin\varepsilon\sin\pi\right\}$$
$$+ a'\sin\varepsilon'\left\{a\cos\varphi\sin\varepsilon\cos\pi + \sin\pi\,(a\cos\varepsilon - ae)\right\}.$$

Putting for brevity

$$a^2(1 - e\cos\varepsilon)^2 = p_\varepsilon$$

$$2\left\{\cos\pi\,(a\cos\varepsilon - ae) - a\cos\varphi\sin\varepsilon\sin\pi\right\} = q_\varepsilon$$

$$2\left\{\sin\pi(a\cos\varepsilon - ae) + a\cos\varphi\sin\varepsilon\cos\pi\right\} = s_\varepsilon$$

we get

$$\rho^2 = (a'^2 + p_\varepsilon) - a'\,(q_\varepsilon\cos\varepsilon' + s_\varepsilon\sin\varepsilon').$$

The differential equation for the secular variation of the perihelion is

$$d\pi = \frac{an\sqrt{1 - e^2}}{(1 + m)e}\cdot\frac{dV}{de}\,dt$$

and also

$$\frac{dV}{de} = -\frac{m'}{4\pi^2}\int d\varepsilon \int \left\{ \frac{\cos\varepsilon}{\rho} + \frac{1 - e\cos\varepsilon}{2\rho^3} \cdot \frac{d(\rho^2)}{de} \right\} d\varepsilon'.$$

If we now put

$$A_\varepsilon = -a\cos\pi - ae\cos\varepsilon\cos\pi + \frac{a\,e}{\cos\varphi}\sin\pi\sin\varepsilon$$
$$-\frac{a(2-e^2)}{\cos\varphi}\sin\pi\sin\varepsilon\cos\varepsilon + 2a\cos\pi\cos^2\varepsilon;$$

$$B_\varepsilon = -a\sin\pi - ae\cos\varepsilon\sin\pi - \frac{a\,e}{\cos\varphi}\cos\pi\sin\varepsilon$$
$$+\frac{a(2-e^2)}{\cos\varphi}\cos\pi\sin\varepsilon\cos\varepsilon + 2a\sin\pi\cos^2\varepsilon;$$

we obtain

$$\frac{dV}{de} = \frac{m'\,a'}{4\pi^2}\int d\varepsilon \left\{ A_\varepsilon \int \frac{\cos\varepsilon'}{\rho^3}\,d\varepsilon' + B_\varepsilon \int \frac{\sin\varepsilon'}{\rho^3}\,d\varepsilon' \right\}$$
$$-\frac{m'\,a'^2}{4\pi^2}\int d\varepsilon\cos\varepsilon \int \frac{d\varepsilon'}{\rho^3}.$$

This expression contains the following integrals:

$$R = \int_0^{2\pi} \frac{d\varepsilon'}{\left\{(a'^2 + p_\varepsilon) - a'\,q_\varepsilon\cos\varepsilon' - a'\,s_\varepsilon\sin\varepsilon'\right\}^{\frac{3}{2}}},$$

$$S = \int_0^{2\pi} \frac{\sin\varepsilon'\,d\varepsilon'}{\left\{(a'^2 + p_\varepsilon) - a'\,q_\varepsilon\cos\varepsilon' - a'\,s_\varepsilon\sin\varepsilon'\right\}^{\frac{3}{2}}},$$

$$T = \int_0^{2\pi} \frac{\cos\varepsilon'\,d\varepsilon'}{\left\{(a'^2 + p_\varepsilon) - a'\,q_\varepsilon\cos\varepsilon' - a'\,s_\varepsilon\sin\varepsilon'\right\}^{\frac{3}{2}}}.$$

which must be computed for every value of ε obtained by dividing the circumference into j parts.

If we put

$$a'^2 + p_\varepsilon = \lambda;\; a'\,q_\varepsilon = q\cos Q;\; a'\,s_\varepsilon = q\sin Q,$$

we get

$$\left\{(a'^2 + p_\varepsilon) - a'\,q_\varepsilon\cos\varepsilon' - a'\,s_\varepsilon\sin\varepsilon'\right\}^{-\frac{3}{2}}$$
$$= \left\{\lambda - q\cos(\varepsilon' - Q)\right\}^{-\frac{3}{2}}$$
$$= a_0^{(3)} + 2a_1^{(3)}\cos(\varepsilon' - Q) + 2a_2^{(3)}\cos 2(\varepsilon' - Q) + \cdots$$

The quantities a_0, a_1, a_2, etc., may be computed by Hansen's formulæ given in his work, Auseinandersetzung, etc., 1 Abth., § 61, and when found R, S, and T are found from the formulæ

$$R = 2a_0\pi\,; \; S = 2a_1 \sin Q\,; \; T = 2a_1 \cos Q.$$

It would be interesting to know what influence the supposed intra-Mercurial planet would have on the other bodies of the system, especially on Venus.

The differential equation of the motion of the ascending node is

$$\frac{d\theta}{dt} = \frac{365.25\, k}{\sqrt{a(1+m)}\sin i}\frac{dV}{di}.$$

The quantity k is the well known Gaussian constant, expressed in seconds of arc, and its logarithm is $\log k = 3.5500065746$.

The value of ρ^2 is expressed by

$$\rho^2 = a^2 + a'^2 - 2aa' \cos(a', a)$$

from which we have

$$\frac{d(\rho^2)}{di} = -\,2aa'\frac{d[\cos(a, a')]}{di}.$$

But

$$\cos(a, a') = \cos\varepsilon \cos\varepsilon' + \sin\varepsilon \sin\varepsilon' \cos I,$$

$$\cos I = \cos i \cos i' + \sin i \sin i' \cos(\theta - \theta'),$$

$$\cot\Phi \sin(\theta - \theta') - \cot i' \sin i = -\cos(\theta - \theta') \cos i.$$

Differentiating we get

$$\frac{d\Phi}{di} = \frac{\sin^2\Phi}{\sin(\theta - \theta')}(\cot i' \cos i + \cos(\theta - \theta')\sin i) = t_1.$$

We also have

$$\frac{d(\cos I)}{di} = -\cos i' \sin i + \cos i \sin i' \cos(\theta - \theta') = t_2.$$

From these expressions we obtain

$$\frac{d(\cos a, a')}{di} = (-\cos\varepsilon' \sin\varepsilon + \sin\varepsilon' \cos\varepsilon \cos I)t_1 + \sin\varepsilon' \sin\varepsilon\; t_2,$$

and therefore $d(\rho^2)$ becomes

$$\frac{d(\rho^2)}{di} = p_\varepsilon \cos\varepsilon' + q_\varepsilon \sin\varepsilon'$$

in which

$$p_\varepsilon = 2aa' \sin\varepsilon\; t_1,$$

and

$$q_\varepsilon = -\,2aa'(\cos\varepsilon \cos I\; t_1 + \sin\varepsilon\; t_2).$$

The expression $\frac{d\theta}{di}$ will in the actual computation be written in the form

$$\frac{d\theta}{di} = \frac{365.25\, k}{\sqrt{a\,(1 + m)}\,\sin i} \frac{m'}{2\pi} \cdot C,$$

in which

$$jC = Tp_{\varepsilon} + Sq_{\varepsilon}.$$

Using the values $\theta' = 14°\,36'$, $a' = 0.200$, $i' = 7°$, and $m' = \frac{1}{2000000}$ derived by me some time since for the intra-Mercurial planet, the perturbation of the node of Venus' orbit is found to be

$$\delta\theta = -\,3.''92120$$

in a century.

Mr. HILL, commenting on this paper, discussed briefly the probable mass and density of Mercury on grounds of probability and analogy, pointing out that its density would on such grounds appear to be far less than the ordinarily accepted value.

18TH MEETING. APRIL 15, 1885.

In the absence of the Chairman, Mr. DOOLITTLE was made Chairman *pro tem.*

Present, ten members and one guest.

Minutes of the sixteenth and seventeenth meetings were read and approved.

Mr. MARCUS BAKER then read the following paper on

A GROUP OF CIRCLES RELATED TO FEUERBACH'S CIRCLE.

In any plane triangle the middle points of the sides and the feet of the perpendiculars drawn from the vertices to the opposite sides are *six* points in the circumference of a circle. This circle also bisects the segments of the perpendiculars between orthocenter and vertices, thus making *nine* noteworthy points. These properties

were first published in January, 1821, in Gergonnes' Annales, vol. II, in a memoir by Brianchon and Poncelet on the determination of an equilateral hyperbola from four given conditions.

In 1822 Prof. K. W. Feuerbach, of Nurenburg, showed that this circle is tangent to the inscribed and three escribed circles. This property is known as Feuerbach's theorem, and the circle is known to the Germans as Feuerbach's circle, In 1828 Steiner showed that this circle passed through twelve noteworthy points and was tangent to the in and escribed circles. This was done without a knowledge of the earlier work by Feuerbach.

In 1842 Terquem, the editor of the *Nouvelles Annales de Mathématiques*, called it the *nine-points circle.* In some books it has been called the *six-points circle.* In an article, by myself, in the Mathematical Magazine for January, 1882, I have called it the *twelve-points circle.*

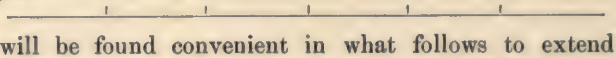
Of the twelve points considered noteworthy six are in the sides of the triangle and six are not. If all the noteworthy points now known are to determine its designation, then *twelve-points circle* appears to be a proper designation. If only those points in the *sides* of the triangle should determine the designation then *six-points circle* would appropriately name it. In either case *nine-points circle* would be an imperfect designation, and as the name Feuerbach's circle was the first name it received it seems on the whole best to adhere to it. A somewhat analogous case is the *seven-points circle* known as Brocard's circle, not named for the number of noteworthy points it contains but for its discoverer. The name *twelve-points circle* may therefore be rejected and the name Feurbach's circle adopted.

The following proof of the fundamental properties of Feuerbach's circle is offered as being simpler than that usually given:

Preliminary.

1. *Definition.* When we determine upon a right line AB a point C such that $AC = BC = \frac{1}{2} AB$ the line is said to be bisected. This restricts us to one point C.

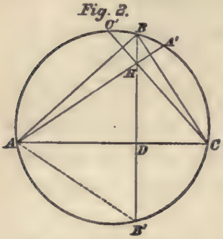
Fig. 1.



It will be found convenient in what follows to extend this definition so as to include the points C' and C'', Fig. 1, where $AC = BC = AC' = BC'' = \frac{1}{2} AB$; *i. e., by considering a defi-*

nite line AB to be bisected when a segment equal to one-half the line is laid off from either extremity in either direction.

Fig. 2.



2. *Theorem.* The perpendiculars of a plane triangle meet in H, and, being prolonged to intersect the circumference in A', B', C', the segments HA', HB' and HC' are bisected by the sides of the triangle. (*See* Fig. 2.)

Proof: $AB'D = AHD =$ angle C.

NOTE.—Here bisection is used in its ordinary sense.

H is the *orthocenter*, and the theorem may be otherwise stated as follows: *The segments of the perpendiculars included between the orthocenter and circum-circle are bisected by the sides of the triangle.*

The point of intersection of the medians is the eidocenter,* which we call G, and the well-known theorem that the medians are concurrent and mutually divided into segments of which the greater is twice the less may be otherwise stated thus: *The segments of the medians included between the eidocenter and vertices are bisected by the sides of the triangle.*

NOTE.—Here bisection is used in its extended sense.

Using bisection in this extended sense it is therefore possible to unite these propositions into a single general statement, as follows:

The segments of the $\left\{\begin{matrix}\text{perpendiculars}\\ \text{medians}\end{matrix}\right.$ between the $\left\{\begin{matrix}\text{orthocenter}\\ \text{eidocenter}\end{matrix}\right.$ and circumcircle measured $\left\{\begin{matrix}\text{away from}\\ \text{towards}\end{matrix}\right.$ the vertices are *bisected* by the sides of the triangle.

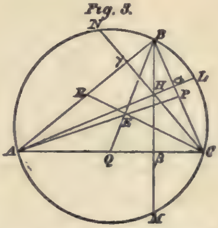
Thus in Fig. 3.

$\alpha H = \alpha L = \frac{1}{2} HL;$ and $EP = \frac{1}{2} EA;$

$\beta H = \beta M = \frac{1}{2} HM;$ $\quad EQ = \frac{1}{2} EB;$

$\gamma H = \gamma N = \frac{1}{2} HN;$ $\quad ER = \frac{1}{2} EC.$

Fig. 3.



3. Conceive the triangle ABC (Fig. 4) and its circumcircle O to be the base of an oblique cone withinscribed tetrahedron. Let the vertex of this cone be so taken that when the whole is projected

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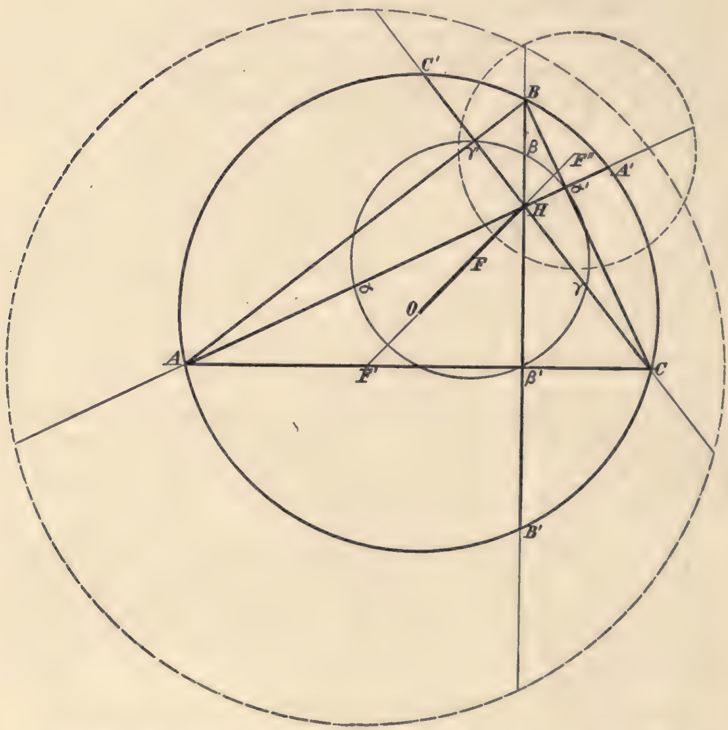
* This term from εἶδος, *form*, and κέντρον, *center*, has been suggested by Mr. Henry Farquhar as being a more accurate derivation than the term *centroid* often used. It is, moreover, analogous to orthocenter, circumcenter, etc.

upon the plane of the base the projection of the vertex shall fall at the orthocenter H. Now let the whole be orthogonally projected upon the plane of the base.

Fig. 4.



From this conception it is seen that HA, HB, HC and HA', HB', HC' are projections of elements of the cone and HO the projection of the axis. Let the axis of the cone be bisected by planes parallel to the base. There are three such bisecting planes: *one* midway between apex and base, *another* below the base, and a *third* above the base. Finally let all these sections be orthogonally projected upon the plane of the base.

From these conceptions it follows immediately that—

(a.) These projections are circles whose diameters are, respectively, $\frac{1}{2}$, $\frac{3}{2}$, $\frac{1}{2}$, the diameter of the circumcircle.

(b.) The centers of these circles F, F', F'', all lie in the line HO,

joining the circumcenter to the orthocenter, and are points of bisection, in the ordinary and extended sense, in such manner that $FH = FO = F'O = F''H = \frac{1}{2}HO$.

(*c.*) The segments of the perpendiculars HA, HB, HC, and HA', HB', HC' are all bisected by each of these circles. In the case of the *first* circle F, the segments are bisected in the ordinary sense, and since the segments HA', HB', and HC' are also bisected by the sides of the triangle this circle *passes through the feet of the perpendiculars.*

The points of bisection on the perpendiculars determined by the circle F are points of bisection in the ordinary sense.

The points of bisection on the perpendiculars determined by the circle $\left\{\begin{matrix} F' \\ F'' \end{matrix}\right.$ are points of bisection in the *extended* sense, and in such wise that the segments cut off from A and A', B and B', C and C' are measured from the orthocenter $\left\{\begin{matrix}\text{towards} \\ \text{away from}\end{matrix}\right.$ the vertices.

(*d.*) Every line drawn from H to the circumcircle is bisected by the three circles F, F', and F''. H is therefore a *direct* center of similitude common to the circles O, F, and F' and an *inverse* center of similitude common to all four circles.

4. The eidocenter is collinear with the circumcenter, orthocenter, and Feuerbach center*; for from a known theorem we have $HB = 2\ OM$, and therefore HO must divide BM into segments of which the greater is twice the less, *i. e.*, it must pass through the eidocenter.

5. Again conceive the triangle ABC (Fig 5) as the base of an oblique cone, etc, as in section 3, except that its vertex is to be conceived as perpendicularly over the eidocenter instead of over the orthocenter, and the whole projected as before. In this case EA, EB, EC, and EA', EB', EC' are projections of the elements of the cone and EO, coincident with HO from section 4, the projection of the axis.

Let the axis be bisected as before by three planes parallel to the base: *one* midway between apex and base, *another* below the base, and a *third* above the apex, and the sections so formed projected upon the plane of the base.

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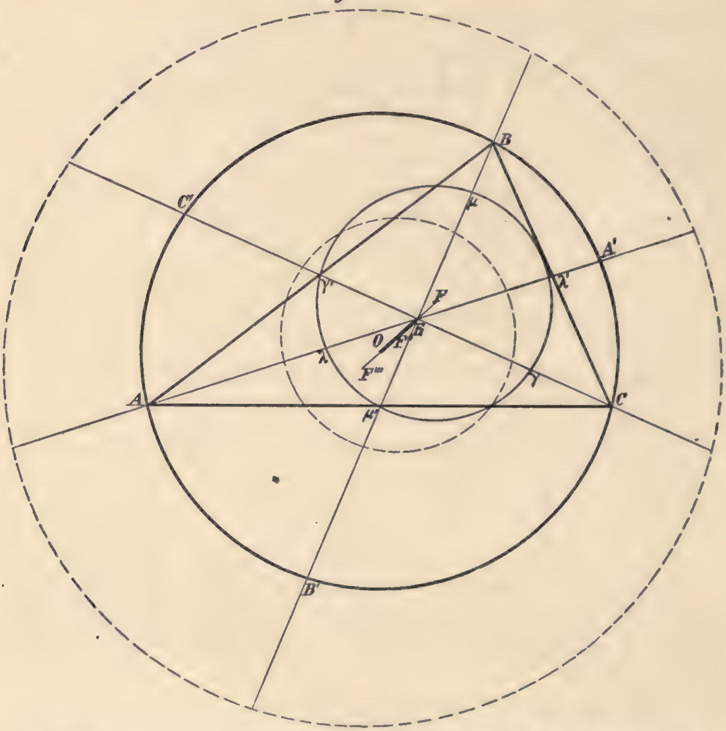
* The center of Feuerbach's circle may be so called for brevity.

We then have—

(*a*.) These projections are circles whose diameters are, respectively, $\frac{1}{2}$, $\frac{3}{2}$, and $\frac{1}{2}$, the diameter of the circumcircle.

Fig. 5.



(*b*.) The centers of the circles F'', F''', F all lie in EO and therefore in HO joining circumcenter to orthocenter and are points of bisection of EO in both the ordinary and extended sense in such wise that $F''\,E = F''\,O = F'''\,O = FE = \frac{1}{2}\,EO$.

(*c*.) The segments of the medians EA, EB, EC and EA', EB', EC' are all bisected by each of the circles. In the case of the *third* circle F, the segments are bisected in the *extended* sense, and since the segments of the medians, EA, EB, EC, are also bisected in the extended sense by the sides of the triangle *the circle F bisects the sides of the triangle.*

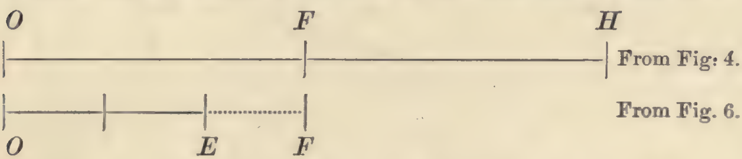
The points of bisection on the medians determined by the circle F'' are bisections in the *ordinary* sense.

The points of bisection on the medians determined by the circles $\left\{\begin{matrix} F''' \\ F \end{matrix}\right.$ are points of bisection in the *extended* sense, and in such manner that the segments cut off from A and A', B and B', C and C' are measured from the eidocenter $\left\{\begin{matrix}\text{towards} \\ \text{away from}\end{matrix}\right.$ the vertices.

(d.) Every line drawn from E to the circumcircle is bisected by the three circles F'', F''', F; E is therefore a *direct* center of similitude common to three circles O, F'', F''' and an *inverse* center of similitude common to all four circles.

6. If we now compare figures 4 and 6 and consider the three circles in each which resulted from projection, we find that circle F, and only circle F, is identical in the two figures.

This identity appears from the fact that their diameters are equal, each equaling half the diameter of the circumcircle, and their centers coincident. The coincidence of their centers appears from drawing the projections of the axes of the cones side by side, thus:



From Fig. 4 we find that this circle, Feuerbach's, passes through 6 noteworthy points, being 2 points on each perpendicular; and from Fig. 6 we find that the same circle passes through 6 other noteworthy points, being 2 on each median, or in all Feuerbach's circle passes through 12 noteworthy points.

7. It is apparent from the foregoing that if we should not bisect the axis of the cone but should cut it in any ratio (Fig. 4) by a plane parallel to the base then the projection of the section would be a circle cutting the perpendiculars in that ratio and its center F_n would divide HO in that ratio. The medians would *not* be divided in that ratio, but a point in HO exists (call it E_n) through which, if lines be drawn from the vertices to the opposite sides, *these* lines would be divided in the given ratio.

The four points O, E_n, F_n, H would in this case, as before, be four harmonic points.

8. Since Feuerbach's circle bisects the sides of the triangle it is the circumcircle of a triangle similar to the original and of half its

size. The results here deduced may therefore be considered to be results of a comparison of the circumcircles of these two triangles. A corresponding study of the relations of the tangent circles (in- and escribed) would therefore be expected to yield many more properties as there are four times as many circles to be considered.

Concerning the phrase "bisection in the extended sense," Mr. DOOLITTLE suggested that the term "sesquisection" might be advantageously employed. As to the name "nine-points circle," Mr. KUMMELL said that it was plainly defective, either "six-points" or "twelve-points" circle being satisfactory, according to the point of view taken, but that nine-points circle was not a correct designation from any point of view. Further remarks were made by Mr. ELLIOTT.

Mr. C. H. KUMMELL presented a communication entitled

DISTANCES ON ANY SPHEROID.

[Abstract.]

The present form of solution of the problem to determine the shortest distance between two points on a spheroid which are given by their latitudes and longitudes is characteristic in making use of the Gaussian algorithm of the arithmetico-geometric mean. This and a corresponding transformation of the amplitudes give the necessary elements for computing in three terms the distance precise to the eighth order *at least.* The form is also remarkable for its symmetry and easy extensibility to still higher precision.

Also the preliminary part of the problem in which the excess of the spherical longitude over the spheroidal is determined by successive approximations is much facilitated by the introduction of an angle η, which is closely related to ε, the angle of eccentricity, and which varies between 0 and ε. It was thus possible to express the excess of spherical over spheroidal longitude in one term, precise to the 6th order *at least.*

[This paper has been published in full in the Astronomische Nachrichten, No. 2671.]

In reply to a question, Mr. KUMMELL said that the ordinary formulæ for computing distances between intervisible points on the terrestrial spheroid are all that can be desired. The formulæ here

presented, however, are designed for much greater distances and for any spheroid, and would serve, if need ever arose, for computing the shortest distance between any two points on the terrestrial spheroid no matter how remote.

19TH MEETING. APRIL 29, 1885.

The Chairman, Mr. G. W. HILL, presided.

Present, nineteen members and one guest.
Minutes of the eighteenth meeting read and approved.

Mr. A. ZIWET read a paper entitled

ON GRASSMANN'S SYSTEM OF GEOMETRY.

This paper will appear in full in the *Annals of Mathematics*, vol. 2, Nos. 1 and 2.

In reply to a question by Mr. Curtis as to whether Grassmann's system could be advantageously substituted for the Cartesian system, Mr. ZIWET expressed the opinion that it could not be so substituted in general, but that it might in certain special cases.

Grassmann has not, said Mr. ZIWET, made applications of his method to astronomy, nor indeed does its value consist in its adaptability to the solution of special problems. But for presenting general geometrical truths it appears superior to Hamilton's methods, to which it is closely related and with which it might be advantageously joined.

Mr. HALL remarked that he had seen planetary orbits worked out after Hamilton's method by J. Willard Gibbs, but the process appeared rather more laborious than the usual Gaussian one.

The labor of computation of results, Mr. HILL remarked, was practically the same by all methods. By introducing the needful symbols the general expressions may be made exceedingly simple, but when the numerical work begins it will be found that after paring off more or less extraneous matter there still remains a central kernel or core of computation from which there is no escape by any method whatsoever.

With this view Mr. R. S. WOODWARD heartily concurred, and added that the supreme test of the usefulness of such systems as

Grassmann's, Hamilton's, etc., consists in their ability to reveal new truths; a test which, according to Mr. Ziwet, Grassmann's system successfully stands.

Mr. M. H. DOOLITTLE presented a communication on

CAUSE AND CHANCE IN THE CONCURRENCE OF PHENOMENA.

The author's views set forth in this communication were stated to be preliminary and incomplete, and he therefore reserves them to be more fully elaborated before publication.

20TH MEETING. MAY 13, 1885.

The Chairman, Mr. G. W. HILL, presided.

Present, eleven members and three guests.

Minutes of the nineteenth meeting, read, corrected, and adopted.

Mr. G. L. RAVENÉ read a paper entitled

THE ASTEROIDS.

This communication elicited a general discussion, participated in by Messrs. KUMMELL, RITTER, BAKER, WOODWARD, ELLIOTT, PAUL, and HILL.

Mr. RITTER then read a paper on

SECULAR PERTURBATIONS OF POLYHYMNIA BY JUPITER.

[Abstract.]

In the computations of these perturbations Gauss's method has been employed, using the formulæ adapted to facilitate the application of this method given by Mr. G. W. Hill.

The eccentricity of Polyhymnia being very large the circumference, with reference to the eccentric anomaly of Polyhymnia, has been divided into twenty-four parts. This is a greater number than necessary, but it seemed worth the additional labor required to have the forces and the other quantities involved for as large a number of points as practicable.

The epoch for both systems of elements is 1873, July, 17.0, Berlin mean time.

The ecliptic and mean equinox are for 1873.0.

The resulting secular variations in one Julian year are the following:

Secular variation of the eccentricity, or			$\delta e = + 1''.268$;
"	"	inclination, or	$\delta i = - \ 1.649$;
"	"	ascending node, or	$\delta \Omega = - 61.031$;
"	"	longitude of the perihelion, or	$\delta \pi = + 59.116$;
"	"	mean longitude, or	$\delta L = - 83.429$;

The paper was discussed by Messrs. PAUL, HILL, and WOODWARD.

21ST MEETING. MAY 27, 1885.

The meeting was called to order at 8:15 by the Chairman, Mr. G. W. HILL.

Seventeen members present.

At the request of the Chair Mr. WINLOCK acted as secretary *pro tem.*, Mr. BAKER being absent.

Mr. R. S. WOODWARD read a paper on

SOME PRACTICAL FEATURES OF A FIELD TIME DETERMINATION WITH A MERIDIAN TRANSIT.

[Abstract.]

An important desideratum in all kinds of field work is the adoption of those methods which will secure the accuracy essential in the results sought with the minimum amount of computation. It is in general easier and more conducive to precision to eliminate unnecessary factors involved with the quantity sought than to determine their values and allow for them by computation. Very frequently also a systematic arrangement of observations will secure the maximum precision with the minimum of computation.

In a field time determination for telegraphic longitude the essential quantity is the error of the time-piece used at some determinate epoch, and the unessential factors are the azimuth and collimation of the transit and the rate of the time-piece. It is evident that a

minimum of computation will be required if the observations can be so arranged as to eliminate the effect of these factors in the final value of the correction to the time-piece. Although it is usually impossible to eliminate the effect of the azimuth, collimation, and rate completely, it is generally possible to make a close approximation thereto. To show this fact analytically let

Δt = the correction to the time-piece at the epoch t_0;
t = the observed time of a star's transit;
α = the star's right ascension;
a = the azimuth of the transit;
c = the collimation of the transit;
r = the rate of the time-piece;
A = the azimuth factor;
C = the collimation factor;
p = the weight of $(t - \alpha)$;
v = the residual error.

Then the type observation-equation will be

$$\Delta t + Aa + Cc + (t - t_0)\, r + t - \alpha = v. \tag{1}$$

The normal equation in Δt, using brackets to indicate summation of like quantities, is

$$[p]\,\Delta t + [pA]\,a + [pC]\,c + [p\,(t - t_0)]\,r + [p\,(t - \alpha)] = 0. \tag{2}$$

This shows that in order to secure the complete elimination of the effect of a, c, and r, we must have

$$[pA] = 0,\ [pC] = 0,\ [p\,(t - t_0)] = 0. \tag{3}$$

The last of these conditions can always be fulfilled by making

$$t_0 = \frac{[p\,t]}{[p]}. \tag{4}$$

It may be shown that the value of Δt corresponding to t_0 as defined by (4) has a maximum weight. A close approximation to the first two conditions of (3) can be secured by selecting for observation stars of suitable declinations and by reversals of the telescope.

If we put

$$\frac{[pA]}{[p]} = \beta,\ \frac{[pC]}{[p]} = \gamma,\ \frac{[p\,(t - \alpha)]}{[p]} = \Delta t_0,$$

equation (2) gives $\Delta t = -\Delta t_0 - \beta\,a - \gamma c.$ (5)

This shows that in case β and γ are small, as supposed above, an

approximate value of Δt is $- \Delta t_0$. After some preliminary observations at a station it is easy to render a and c small, and their approximate values may always be found from the observation equations by a brief inspection; so that with such values of a, c, β, and γ as are nearly always readily attainable Δt may be derived from (5) to the nearest $0^s.01$.

We may thus dispense entirely with the other three normal equations and reach the same result which would follow from their use. The solution may also be checked; for by one or two approximations the values of Δt, a and c which make $[pv] = 0$ can be readily found.

The practical steps in deriving Δt from (5) may be summarized as follows:

1. The mean of the observation-equations for clamp west minus the mean of those for clamp east will give an approximate value of the collimation c.

2. The application of this value of c to each observation equation will give a corrected value of $(t - a)$ for each star.

3. An approximation to the value or values of the azimuth will then result by eliminating Δt from one or more pairs of the corrected observation equations. The azimuth may then be applied to correct the values of $(t - a)$, reached in step 2.

4. The approximate values of a and c will now give an approximate value of Δt from (5), and the application of this value of Δt to the values of $(t - a)$, derived in step 3, will give approximate values of v.

5. Form $[pv]$. If this sum is not zero within $0^s.01$ or $0^s.02$, a brief inspection will show what changes in a and c (and possibly Δt) will make it zero within those limits.

By this process of determining the residuals or their approximate values as soon as possible in the computation any large errors in the values of $(t - a)$ or the azimuth and collimation factors will be easily detected.

In precise longitude determinations it is customary to have for each night's observations two complete time determinations, one immediately preceding and one immediately following the telegraphic comparison of time-pieces. In this case there will be two values of Δt. Calling these $\Delta t'$ and $\Delta t''$ and denoting the corres-

ponding epochs by t_0' and t_0'' the rate of the time-piece will be given with sufficient accuracy for interpolation by the equation

$$r = \frac{\Delta t'' - \Delta t'}{t_0'' - t_0'}.$$

Mr. PAUL thought it an objection to this method that in arranging the groups valuable stars might be lost, so that in a limited time the accuracy of the results would be impaired by the smaller number of observations; moreover, the method did not furnish the computer with a clear idea of the performance of the instrument.

Mr. HALL said that he liked the method, and that he thought it especially good for time work. He had discovered the method once himself, and he knew that it had also been used by Prof. Ormond Stone.

Mr. KUMMELL said that in connection with this subject he had investigated the question of the advisability of using stars towards the pole for time determinations; that is, he had examined the weight co-efficient formulæ to see at what distance north of the zenith a maximum value would be obtained. He found that in general the limit of declination was about 60°.

Mr. PAUL thought that every weight-formula should take account of the increase of atmospheric disturbance with increase of zenith distance.

Mr. KUMMELL then read the following paper entitled

CAN THE ATTRACTION OF A FINITE MASS BE INFINITE?

In Price's Calculus, vol. III, art. 201, discussing the result for the attraction of a thin rectangular plate on a particle external to it and in its own plane it is found that if the attracted particle is at an angle of the rectangle the attraction is infinite. Price's method of determining the attraction of plates consists in integrating between the proper limits the following differentials:

$$d^2X = m\theta\tau \frac{x\,dx\,dy}{(x^2 + y^2)^{\frac{3}{2}}}; \qquad (1_x)$$

$$d^2Y = m\theta\tau \frac{y\,dx\,dy}{(x^2 + y^2)^{\frac{3}{2}}}; \qquad (1_y)$$

where $X = x$ — axial component of attraction;

$Y = y$ — axial component of attraction;

m = mass of attracted particle;

θ = density of attracting mass, supposed homogeneous;

τ = thickness of the plate.

Referring to the attracted particle as origin let (a_0, b_0) be the corner nearest and (a_1, b_1) that farthest from the particle; then

$$X = m\theta\tau \int_{b_0}^{b_1} dy \int_{a_0}^{a_1} \frac{xdx}{(x^2+y^2)^{\frac{3}{2}}}$$

$$= m\theta\tau \int_{b_0}^{b_1} dy \left((a_0^2 + y^2)^{-\frac{1}{2}} - (a_1^2 + y^2)^{-\frac{1}{2}} \right)$$

$$= m\theta\tau l \left\{ \frac{b_1 + (a_0^2 + b_1^2)^{\frac{1}{2}}}{b_0 + (a_0^2 + b_0^2)^{\frac{1}{2}}} \cdot \frac{b_0 + (a_1^2 + b_0^2)^{\frac{1}{2}}}{b_1 + (a_1^2 + b_1^2)^{\frac{1}{2}}} \right\} \qquad (2)$$

and a similar expression for Y by exchanging the a's for the b's.

If in this we place $a_0 = b_0 = 0$ then $X = m\theta\tau\,\infty$.

This is taken by Price to be infinite; yet, since the thickness τ must be taken infinitesimal, this is an entirely unfounded conclusion.

At first, however, I did not suspect this result, and when Mr. Woodward found an infinite attraction of a circular disk on a point at its circumference, which result I checked, it seemed to be possible that the attraction of a finite mass could be infinite. Yet neither Mr. Woodward nor myself was entirely convinced. To settle this question I then resolved to determine, the attraction of a right prism and also of a right circular cylinder on a particle at mid-height, which, being then moved to the surface and taking the height infinitesimal, would give the attraction of a plate on a particle in a position at which an infinite attraction had been found.

For a right rectangular prism we have, $2h$ being its height,

$$X = m\theta \int_{b_0}^{b_1} dy \int_{a_0}^{a_1} xdx \int_{-h}^{h} \frac{dr}{(x^2 + y^2 + z^2)^{\frac{3}{2}}}$$

$$= m\theta \int_{b_0}^{b_1} dy \int_{a_0}^{a_1} \frac{2hxdx}{(x^2 + y^2)\,(x^2 + y^2 + h^2)^{\frac{1}{2}}} \qquad (3)$$

Comparing this with (2), putting $2h = \tau$, we readily see that these values are by no means identical, for (2) is of the form $\tau f_2\,(a_1, a_0, b_1, b_0)$ while (3) is $\tau f_3\,(a_1, a_0, b_1, b_0, \tau)$. Here f_2 is independent of τ while f_3 is not.

Placing $a_0 = b_0 = o$ or supposing the attracted particle at the edge of the prism we have

$$X = 2m\theta h \int_o^{b_1} dy \int_o^{a_1} \frac{xdx}{(x^2 + y^2)\,(x^2 + y^2 + h^2)^{\frac{1}{2}}}$$

$$= 2m\theta h \int_o^{b_1} dy \int_{\sqrt{y^2 + h^2}}^{\sqrt{a_1^2 + y^2 + h^2}} \frac{dr}{z^2 - h^2}$$

$$= 2m\theta h \int_o^{b_1} dy \left[\frac{1}{2h}\, l\, \frac{z - h}{z + h}\right]_{\sqrt{y^2 + h^2}}^{\sqrt{a_1^2 + y^2 + h^2}}$$

$$= m\theta \int_o^{b_1} dy \left[\, l\, \frac{z - h}{z + h}\right]_{\sqrt{y^2 + h^2}}^{\sqrt{a_1^2 + y_2 + h^2}} \qquad (4)$$

This form shows clearly that X vanishes with h; therefore the attraction of a rectangular plate on a particle at one of its corners is not infinite, but, on the contrary, it is infinitesimal.

Similarly the attraction of a circular plate on a particle at its circumference may be found by considering the plate a vanishing cylinder. Using cylindrical co-ordinates to a vertical line through the attracted particle as axis we have, if $\rho =$ horizontal radius vector, $v =$ vectorial angle, for the attraction of a cylinder, radius r, density θ, on a particle at midheight h and distance d from axis of cylinder where

$$\cos v = \frac{\rho^2 + d^2 - r^2}{2\rho d}$$

$$\text{and } \sin v = \frac{1}{2\rho d} \sqrt{[(d + r)^2 - \rho^2]\,[\rho^2 - (d - r)^2]}$$

$$A = m\theta \int_{d-r}^{d+r} \rho^2\, d\rho \int_{-v}^{v} \cos v dv \int_{-h}^{h} \frac{dr}{(\rho^2 + z^2)^{\frac{3}{2}}}$$

$$= m\theta \int_{d-r}^{d+r} \rho^2\, d\rho \cdot 2 \sin v \,.\, \frac{2h}{\rho^2 \sqrt{\rho^2 + h^2}}$$

$$= 2m\theta\,\frac{h}{d}\int_{d-r}^{d+r}\frac{d\rho}{\rho}\sqrt{\frac{[(d+r)^2-\rho^2][\rho^2-(d-r)^2]}{\rho^2+h^2}}$$

$$= 2m\theta\,\frac{h}{d}\int_{\sqrt{b^2-h^2}}^{\sqrt{a^2-h^2}}\frac{d\rho}{\rho}\sqrt{\frac{(a^2-\rho^2-h^2)(\rho^2+h^2-b^2)}{\rho^2+h^2}}$$

where $a = \sqrt{(d+r)^2+h^2}$; and $b = \sqrt{(d-r)^2+h^2}$ (5)

Assume $\tan\varphi = \frac{a}{b}\sqrt{\frac{\rho^2+h^2-b^2}{a^2-\rho^2-h^2}}$ then φ has the limits $\lrcorner$ and 0. We have then $\rho^2+h^2 = \frac{a^2\,b^2}{a^2\cos^2\varphi+b^2\sin^2\varphi} = \frac{b^2}{\Delta\varphi^2}$, where $\Delta\varphi$ is an elliptic Δ function to the modulus $\sqrt{1-\frac{b^2}{a^2}}$. Transforming we have, expressing in Δ functions

$$A = 2m\theta\frac{h}{d}\int_0^{\lrcorner} ab^2\,\frac{d\varphi}{\Delta\varphi^3}\cdot\frac{(1-\Delta\varphi^2)\left(\Delta\varphi^2-\frac{b^2}{a^2}\right)}{b^2-h^2\,\Delta\varphi^2}$$

$$= 2m\theta\frac{ab^2}{hd}\left\{-\frac{h^2}{a^2}\int_0^{\lrcorner}\frac{d\varphi}{\Delta\varphi^3}+\int_0^{\lrcorner}\frac{d\varphi}{\Delta\varphi}-\frac{(d^2-r^2)^2}{a^2\,b^2}\int_0^{\lrcorner}\frac{d\varphi}{\left(1-\frac{h^2}{b^2}\,\Delta\varphi^2\right)\Delta\varphi}\right\}$$

$$= 2m\theta\frac{ab^2}{hd}\left\{\int_0^{\lrcorner}\frac{d\varphi}{\Delta\varphi}-\frac{h^2}{b^2}\int_0^{\lrcorner}d\varphi\,\Delta\varphi\right.$$

$$\left.-\frac{(d+r)^2}{a^2}\int_0^{\lrcorner}\frac{d\varphi}{\left(1+\frac{4rdh^2}{a^2(d-r)^2}\sin^2\varphi\right)\Delta\varphi}\right\}$$

$$= 2m\theta\,\frac{ab^2}{hd}\left\{F-\frac{h^2}{b^2}\,E-\left(\frac{d+r}{a}\right)^2 \Pi\left(\frac{4rdh^2}{a^2(d-r)^2}\right)\right\} \qquad (6)$$

where F, E, and Π denote quadrantal elliptic integrals of the 1st, 2d, and 3d species, according to Legendre's notation. If our object was to obtain here a formula for convenient computation of the attraction the third term could still be expressed in integrals of the 1st and 2d species, which have been tabulated by Legendre.

Let us now move the particle to the surface of the cylinder, then we have $d = r$; $a = \sqrt{4r^2 + h^2}$; $b = h$, and (6) becomes

$$A' = 2m\theta \frac{ah}{r}(F - E). \tag{6'}$$

Mr. Woodward, as already stated, had found an infinite attraction of a circular disc on a particle on its circumference by using Price's method. Now, since A' is *surely* a finite quantity, we have here the manifest absurdity that the attraction of a cylinder on a particle on its surface would be less than that of a circular disk which is only an infinitesimal part of it.

If we wish to ascertain the attraction of a circular disk of finite small thickness we may, of course, use (6'), and since then F tends to infinity, E may be neglected; therefore

$$\begin{aligned}
A'_{h=o} &= \frac{2m\theta}{r}\left[ah \int_o^{\lrcorner} \frac{d\varphi}{\sqrt{1 - \frac{4r^2}{a^2}\sin^2\varphi}}\right]_{h=o} \\
&\doteq 4m\theta \left[\frac{\displaystyle\int_o^{\lrcorner} \frac{d\varphi}{\sqrt{1 - \frac{4r^2}{a^2}\sin^2\varphi}}}{\frac{1}{h}}\right]_{h=o} \\
&= 4m\theta \left[\frac{\displaystyle -\int_o^{\lrcorner} \frac{4r^2 \sin^2\varphi \frac{da}{dh}}{a^3 \Delta\varphi^3} d\varphi}{-\frac{1}{h^2}}\right]_{h=o} \\
&= 4m\theta \left[\frac{\displaystyle \frac{4r^2 h}{a^4} \int_o^{\lrcorner} \frac{\sin^2\varphi \, d\varphi}{\Delta\varphi^3}}{\frac{1}{h^2}}\right]_{h=o} \\
&= 0
\end{aligned}$$

Since then the attraction is 0 if $h = 0$ it will be small if h is small, and will continuously grow with h.

The notion of an infinite attraction exerted by a finite mass has thus been dispelled in two special cases in which it seemed to have been proved, and I add some remarks on its general impossibility.

The attraction of a mass element on a particle at distance ρ is

$$m\theta \frac{dx\ dy\ dz}{\rho^2}$$

This is then an infinitesimal of the third order, and the summation of its components with respect to a fixed direction requires three integrations, which give surely a finite result if ρ is always finite.

If ρ is infinitesimal there will be *one* element of attraction, which, instead of being of the third order, is only of the *first* order of infinitesimals, and this one element being added to the finite sum of the other elements has no effect. Hence I conclude that a finite mass exerts only a *finite* attraction on a particle.

The paper was discussed by Messrs. HALL, HILL, and WOODWARD. Mr. WOODWARD said that he had arrived at a result similar to Mr. Kummell's by a somewhat different route. The fallacy in Price's Calculus arose from neglecting the thickness of the plate.

NOTE.

The communications and abstracts printed in the proceedings of the Mathematical Section have each been examined by a special committee consisting of the Chairman, the Secretary, and a third member appointed by the Chairman. These third members were as follows:

Title.	*Author.*	*Third Member.*
Physical observations of Wolf's comet	W. C. Winlock.	— — —
The Theory of Mercury	G. L. Ravené.	E. B. Elliott.
A group of circles related to Feuerbach's circle	Marcus Baker.	C. H. Kummell.
Some practical features of a time determination	R. S. Woodward.	H. Farquhar.
Can the attraction of a finite mass be infinite ?	C. H. Kummell.	R. S. Woodward.

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